

JOURNAL OF THE INSTITUTION OF CIVIL ENGINEERS.

No. 8. 1938-39.
OCTOBER 1939.

EXTRA MEETING.

6 June, 1939.

WILLIAM JAMES EAMES BINNIE, M.A., President,
in the Chair.

The President said that those present would feel indebted to Dr. Stradling for coming to deliver his Lecture, which was one of a series arranged by the Council to be delivered in June, in order that members and others might be informed of the latest developments in A.R.P. practice as affecting engineers.

Dr. Stradling was so well known for his work as Director of the Building Research Station that no introduction was necessary. He had taken up his present post as Chief Advisor (Research and Experiment) to the A.R.P. Department of the Home Office in order that his special knowledge might be utilized in connexion with the research and experimental work now being carried on by the Home Office.

Before calling on Dr. Stradling to deliver his Lecture, he would like to offer a special welcome to members of local authorities and other engineering institutions who were present that evening.

“Experimental Work on A. R. P.”

By REGINALD EDWARD STRADLING, C.B., M.C., D.Sc., M. Inst. C.E.

(Abridged Report¹.)

Dr. Stradling referred first to the difficulties experienced in dealing with a subject in a Lecture, which arose from the necessity for secrecy, as it was important that possible opponents should not be made aware of

¹ Copies of the Lecture may be obtained on application to the Secretary.

the whole basis of reasoning on which protection was based, since they would then be in a more favourable position to adjust their methods of attack. Further, it was not possible to obtain the actual type of armament likely to be employed by an enemy, and so experimental work had to be based on information from domestic sources. If exact scientific measurements were published in terms of Service weapons, it was clear that a potential enemy was being presented with exact details of British defence. He hoped, however, to show how extensive the research field really was and to make quite clear that every effort was being made to apply modern scientific knowledge to the problems of civil defence.

After referring to the creation some months ago of the special Research and Experiment Branch of the A.R.P. Department of the Home Office and to the recent appointment of a Civil Defence Research Committee under the Chairmanship of Dr. E. V. Appleton, Secretary of the D.S.I.R., which would ensure that the full resources of the scientific world would be enlisted in the services of that section of Government activity, he dealt with the question of protection from the effects of the high-explosive bomb. On detonation a very high pressure was produced, which caused the metal case to expand to possibly one-and-a-half times its original size and then to burst into fragments. In addition to the formation of splinters, the expanding gases had two effects: firstly, there was an actual movement of gas giving the effect of a very high "wind", and secondly, a wave was propagated, which was similar to a sound-wave. Dealing with the first effect, which caused major destruction but on a surprisingly local scale, Dr. Stradling pointed out that at a comparatively short distance, say about 30 feet, from a large bomb, that effect had practically disappeared. It was usual to regard anything within the "wind" zone as virtually a direct hit. Outside that zone there was the effect of the acoustic type of wave, and that could be spread over a very wide area. Its effect on a structure could be disastrous, but more especially on those portions which had a high natural frequency, such as windows and the like. Experiments showed that the effect on a structure was dependent upon the structure itself, as well as upon the form of the wave. Measurement of the form of those acoustic waves was by means of a piezo-electric gauge taking advantage of the well-known property of the production of an electric charge by pressure. The recording instrument was a cathode-ray oscillograph, the spot on which was photographed by a sensitized film driven by clockwork. After curves had been obtained it was still necessary to interpret them in terms of the effects on structures, and that had been carried out mathematically.

Experiments were in hand to investigate the effect of that type of blast upon the living organism, although all the evidence so far available indicated that nothing was to be feared from the purely physiological aspect except within the "wind" zone. Pressure inside shelters appeared never to be greater than 5 lb. per square inch, and a common rule was that

the maximum pressure to which a gunner could be safely subjected in a position around a gun was 5 lb. per square inch.

Due to the adoption of basements as shelters, the question of earth movements around an exploding bomb was also of importance. There was a zone around the bomb in which few normal structures could be expected to stand, but that was very limited in extent and might be considered as a direct hit. The wave which was effective at longer distances was somewhat similar to a very slight earthquake and had little effect upon a normal building. The effects would depend upon the type of soil, as well as upon the strength of the walls of the basements; the more rigid the walls the greater would be the force exerted upon them. Experiments showed, however, that there was a very rapid decrease in pressure for a small increase in distance.

In considering the effect of splinters, the fragments of the case were projected with great velocity in all directions, and at 50 feet from a fairly large bomb the velocity could be assumed to be about 4,000 feet per second. It was for that reason that the normal standards visualized for rifle bullets were not adequate to give protection, and that the thicknesses stated in the Codes issued by the Departments were apparently so great.

In conclusion, Dr. Stradling referred to the application of some of the above data in a more general way and to the three well-known types of shelter most generally adopted, namely the corrugated steel or "Anderson" type, the domestic surface or "pill-box" type, and the strutted basement type, which were designed on the basis of the experimental work carried out. Reference was also made to accommodation in shelters, and it was pointed out that the three criteria by which the accommodation could be judged were floor-space, cubic content, and interior surface area.

A vote of thanks to the Lecturer was proposed by Mr. F. C. Cook and seconded by Mr. B. D. Richards, and was carried by acclamation.

EXTRA MEETING.

12 June, 1939.

WILLIAM JAMES EAMES BINNIE, M.A., President,
in the Chair.

The President said that they were indebted to Colonel Wyatt for coming to give the second of the series of three Lectures on the latest developments in A.R.P. practice. Colonel Wyatt had been responsible for organizing camouflage during the Great War and had been in charge of that branch of work at G.H.Q., so he could speak with authority on the subject.

"Camouflage."

By COLONEL FRANCIS JOSEPH CALDWELL WYATT, O.B.E., M.C.
(late R.E.).

(*Abridged Report*¹.)

Colonel Wyatt said that the word "camouflage" was used in French theatrical circles to mean "make-up", and had been adopted into the English language during the Great War, when French ideas of hiding guns and observation posts had been copied. Camouflage in some form or another was as old as war itself, as it included all stratagems adopted to deceive the enemy. During the Great War and a few years afterwards camouflage of mobile objects was studied intensively, but not much had been done with regard to the camouflage of factories, depots, and similar objectives.

Owing to the vagaries of the British climate, it had been decided to save time by experimenting on models first and then applying what had been learnt on some selected installations. The purpose of camouflage was to make the bombing of a specific target as difficult as possible. It was sometimes argued that it was a waste of time to make a target inconspicuous if it were near some feature, such as a river or railway, that could not be concealed. It had to be remembered, however, that the raider had to come a long way under probably harassing conditions into an unfamiliar countryside, and anything done to make his task still more difficult was distinctly worth while.

¹ Copies of the Lecture may be obtained on application to the Secretary.

The main causes of easy recognition could be summarized as :—

- (1) The large homogenous expanse of roof having a “ploughed field” effect, due to shadows in the valleys of the roof; that was particularly noticeable in the case of a north-light type of roof.
- (2) The reflection of light by smooth surfaces.
- (3) The bulk.
- (4) The regularity of shadow and silhouette.

The question of regular shadows was the most difficult problem of all. The major problem arose when a large compact factory covered an area of square mile or more, and, was, in fact, so large that it could be said to have no surroundings.

In analysing the effects, there were two main features to attack. The first was form—that was to say, the form of the factory had to be moulded into something which looked like the surroundings. The second was colour or tone. Camouflage had to destroy the characteristic forms of the installation and to reduce them to shapes and tones in harmony with the prevailing pattern of the neighbourhood. There were two principal methods at work in living creatures: firstly, imitation, and secondly, disruption, where the creature presented bold and contrasting patterns, one of which arrested attention and diverted it from the rest of the form.

By an intelligent use of the principles of disruption, attention could be distracted from salient features such as corners and ridges. That was done by introducing on, or near them, a striking contrast in tone, taking care to employ one that was in harmony with the neighbourhood. There was, of course, a complementary method—that was, the addition of excrescences to distort straight lines. Whilst perfectly sound in theory, the engineering problems involved, generally speaking, made it impracticable.

In connexion with the disadvantages of the reflection of light from the smooth surfaces of roofs, the question of producing matt surfaces in a practical way was receiving much attention. There were two solutions to that problem: firstly, coloured grit or fluff could be sprayed on to a sticky surface in two operations; secondly, grit could be first mixed with paint and put on in one operation.

In conclusion, Colonel Wyatt referred to the subject of materials, which was always of paramount importance in engineering, and to the fact that very little skill was required in camouflage painting.

A vote of thanks to the Lecturer was proposed by Mr. R. G. Hetherington and seconded by Dr. Herbert Chatley, and was carried by acclamation.

EXTRA MEETING.

20 June, 1939.

SYDNEY BRYAN DONKIN, Past-President, in the Chair.

The Chairman said that the Lecture that evening was entitled "The Design of Bomb-Proof Shelters", and it would be given by Dr. David Anderson, a Member of Council of The Institution and Chairman of the Design Panel, Engineering Precautions (Air Raids) Committee. It should be emphasized that the subject-matter of the Lecture had no connexion whatever with the policy of shelters or the putting down of the various kinds of such shelters, but only with the design of them. The present Lecture formed the third of a series arranged by The Institution.

Dr. Anderson was well known to all the members of The Institution, but others might be reminded of what he had done and was doing. He was a consulting engineer of renown and experience, having been connected with the Mersey tunnel, the Dartford tunnel, and the new tunnels for the London Passenger Transport Board. He had, also, specialized knowledge of such work as was involved in shelter design.

The Design Panel, of which he was Chairman, of the Engineering Precautions (Air Raids) Committee had been formed by The Institution with the addition of representatives nominated by the Institutions of Mechanical, Electrical, and Structural Engineers, and by the Royal Institute of British Architects. It was formed at the special request of the Home Office to study the information contained in the new A.R.P. Handbook No. 5, which had been issued during the past week, and the information which Dr. Anderson would give in his Lecture that evening would mainly be the subject-matter of a new handbook which would shortly be issued¹.

"The Design of Bomb-Proof Shelters."

By DAVID ANDERSON, LL.D., B.Sc., M. Inst. C.E.

(*Abridged Report* ².)

Dr. Anderson said that the Committee had considered the matter in considerable detail and had prepared altogether about thirty provisional designs, including the three which had been completed for inclusion in

¹ The full Report has been published by the Home Office as Structural Handbook 5A, copies of which can be obtained from H.M. Stationery Office.—SEC. INST. C.E.

² Copies of the Lecture may be obtained on application to the Secretary.

their Report. Attack by bombs might take various forms and be of varying intensity, but in order to be definite in their recommendations the Committee had laid down four types of protection, namely :—

Type 1 Protection. Based on resistance to blast and splinters, debris loads and small incendiary bombs.

Type 2 Protection. Based on resistance to the direct hits of medium weight incendiary bombs and high-explosive bombs.

Type 3 Protection. Designed to give protection against the effects of heavy high-explosive bombs. The standard adopted was considered to be adequate against the effects of a medium-case bomb of the order of 500 lb. weight striking at its maximum velocity. It was also considered to be proof against light-case bombs of considerably greater weight.

Type 4 Protection. Similar to Type 3 Protection, but designed to give protection against the effects of a heavy-case bomb.

Type 1 had already been standardized. Types 3 and 4 were considered to be the most urgently required, and the Report dealt almost entirely with those two, Type 2 being reserved for later consideration.

The designs were based upon the shelters being divided into compartments with not more than 100 persons in each compartment; where more than 1,200 persons had to be protected it was recommended that the shelters should be spaced at least 25 feet apart or some special arrangements made.

The shelters designed consisted essentially of two-storey structures of a box form, either rectangular or circular, formed of reinforced concrete and placed generally half above and half below ground, easy access to the basement storey being obtained by means of staircases. Shelters placed at a considerable depth below ground were not considered in detail, although it was realized that there should not be any great difficulty in designing them to provide degrees of protection similar to those afforded by the recommended designs.

The action of a 500-lb. bomb on a concrete shelter had been studied in detail, and, to meet the combined effect of direct impact, disruptive force of explosion and spalling effect on the inner surface, the thickness of the concrete decided upon was 5 feet for the roof; 3 feet 3 inches for walls above ground, and 6 feet 6 inches below ground; and 6 feet 6 inches for the base, except in large shelters where under certain conditions the thickness could be progressively reduced to 2 feet 6 inches. For type 4 protection the thickness of the roof was increased to 7 feet 6 inches to give greater protection. Alternative methods of protecting the base by carrying down the side walls to form a curtain, or by providing slabs on the ground to form an apron, were considered, but were found to be too costly in most cases.

The class of concrete to be adopted, the nature of the steel reinforce-

ment, the internal lining to resist spalling, and a number of practical details were all investigated, and appropriate standards recommended. Particular study was made of the width of entrance required, etc. In the case of the shelter to accommodate 1,200 persons, the entrances had been arranged so that the shelters could be occupied within $1\frac{1}{4}$ minute.

The Committee took steps to compare their recommendations with the regulations adopted in other countries, and found them to be in general agreement. Estimates were prepared of the cost per person for the various provisional designs, and those costs were found to range from £17 to £66 per person. The costs of the designs that accompany the Report were found to be :—about £25 9s. 0d. per person for the rectangular shelter for 200 persons ; about £21 8s. 0d. per person for the circular one, and about £17 10s. 0d. per person for the large shelter for 1,200 people.

A vote of thanks to the Lecturer was proposed by Sir CLEMENT HINDLEY, President-Elect, who stated that when one looked back on the somewhat difficult history of air-raid shelters, he thought it would be agreed that a great sense of relief should be felt that the work of designing shelters was at last in the hands of competent people, and that the Government and the administrators, who so often thought that they could do everything without technical assistance, had gradually come to the view that the work was work for engineers, and had put it in the hands of engineers. They were fortunate in having Dr. Anderson and his colleagues on the Panel available for translating the valuable information in Handbook No. 5 into a form in which it was of practical use to the engineer.

Mr. F. M. G. Du-Plat-Taylor seconded the vote of thanks, which was passed by acclamation.

EXTRA MEETING.

27 June, 1939.

Sir JOHN EDWARD THORNYCROFT, K.B.E., Vice-President-Elect,
in the Chair,

supported by

Major-General GEORGE BRIAN OGILVIE TAYLOR, C.B.E.,
Director of Fortifications and Works.

Sir John Thornycroft said that they were grateful to Brigadier C. A. Bird for finding the time to come and lecture on "The Work of the Military Engineer in War", a subject which was of great interest to civilian engineers and especially to those who were about to be called up for their militia training. From his experience as Chief Engineer of the Aldershot Command and as a Chief Instructor at the School of Military Engineering, Chatham, and as one who had passed through the Staff College, Brigadier Bird was particularly well fitted to deal with such a subject.

Before calling on the Lecturer, he would like to welcome the Territorial Officers and representatives from the O.T.C. at Westminster and St. Paul's who were present that evening.

"The Work of the Military Engineer in War."

By Brigadier CLARENCE AUGUST BIRD, D.S.O.
Chief Engineer, Aldershot Command.

(Summary.)

Brigadier Bird referred first to engineer organization, tracing it down from General Headquarters to the division, and then to the engineer units allotted to the formations mentioned. He described the work of the Corps Bridge Company, and showed a number of lantern-slides of the equipment used as well as of other engineering equipment. He dealt with types of military engineering work, and after considering the application of certain principles, common to engineering work generally, to those types, he concluded with some remarks on staff work and organization and on the psychological aspect in war.

A vote of thanks to the Lecturer was proposed by Mr. R. G. Hetherington and seconded by Mr. C. D. C. Braine, and was carried by acclamation.

THE DUGALD CLERK LECTURE, 1938*.

“Sir Dugald Clerk and the Gas Engine : His Life and Work.”

By WILLIAM ALFRED TOOKEY, M. Inst. C.E.

(Abridged Report †.)

Mr. Tookey, after stating that he greatly appreciated the privilege of delivering the first Dugald Clerk Lecture to the Students of The Institution, referred to his first introduction to Dugald Clerk at the close of the Annual Dinner of the Junior Institution of Engineers in February 1906, of which Institution he was then President. From that time, at more or less frequent occasions until his death in 1932, opportunities occurred of discoursing on a subject which was of professional interest to both, and he was glad to take the opportunity to express Sir Dugald's interests and consideration for the well-being and professional advancement of one of a generation later than his own.

Dugald Clerk began his mechanical training in 1869 when he was 15 years of age, and that year was spent in the drawing office of H. O. Robinson & Co., Engineers, of Glasgow, where he learnt mechanical drawing and designing. In the following years, 1870-71, he was engaged in his father's machinist workshop in Glasgow, and by the end of 1871 he had obtained pass certificates from the Science and Art Department, London, in about seventeen subjects, including chemistry, heat, light, sound, magnetism and electricity, mathematics, machine drawing and construction. Having formed the idea of becoming a chemical engineer, he attended evening lectures at Anderson's College, Glasgow—afterwards the West of Scotland Technical College—by Professor T. E. Thorpe, D.E., F.R.S., who later became the leading Government Chemist in London.

In the beginning of 1872 Professor Thorpe offered him an assistantship, which he gladly accepted ; and he was set to work on the fractionating of paraffin oils. The 6 months' work that he then did in distilling paraffin oils, and in separating one oil from another, was to him one of the most illuminating experiences that could possibly be imagined. That gave him a fund of knowledge on the question of oils for oil engines that in later

* This Lecture was delivered at a meeting of the Association of London Students and was repeated before the Glasgow, Manchester, Northern Ireland, Southern, and Yorkshire Associations.

† Copies of the Lecture may be obtained on loan from the Loan Library of the Institution ; a limited number of copies is also available, for retention by members on application to the Secretary.

years caused surprise to his engineering friends. He observed that chemistry was invaluable to the engineer, and questioned whether the internal-combustion engine would be in its present position as to scientific development but for the application of chemical principles.

After describing his work with Mr. Louis Sterne, Mr. Tookey referred to the engine patented by Dugald Clerk in 1881, known subsequently throughout the world as the Clerk cycle (two-stroke) engine. During 1881 Dugald Clerk put together material for the Paper he was to read before The Institution on "The Theory of the Gas Engine"¹, in which he was able for the first time to apply his chemical knowledge of the phenomena of combustion. In his investigations he had some extremely interesting results, which he thought he might claim had a distinct effect on the development of that type of engine. That was due entirely, he indefatigably asserted, to his early training in chemistry.

In 1886 he took up a position with the firm of Tangyes Limited, at Birmingham, and in 1888 he left to form a partnership with George Croydon Marks, another employee of that firm and his junior in age by 4 years, as consulting engineers and patent agents. That successful partnership was soon extended to London and afterwards to Manchester. In 1892, Dugald Clerk became an engineering director of Kynoch Limited, until his London practice became too great for him to attend the weekly meetings in Birmingham. In 1902 he accepted an invitation to join the Board of the National Gas Engine Company of Ashton-under-Lyne.

Dugald Clerk made many contributions to technical literature. In 1904, he delivered the James Forrest Lecture on "Internal-Combustion Motors"², and in 1920 that on "Coal Conservation in the United Kingdom"³. He was President of many Institutions, and in 1932 was elected President of The Institution of Civil Engineers, but ill-health prevented him from taking office.

¹ Minutes of Proceedings Inst. C.E., vol. lxxix (1881-82, Part III), p. 220.

² *Ibid.*, vol. clviii (1904-05, Part IV), p. 276.

³ *Ibid.*, vol. ccx (1919-20, Part II), p. 268.

Paper No. 5196.

“The Direct Design of Redundant Frameworks, With or Without Initial Primary Stresses.”

By RICHARD BORRETT WHITTINGTON, M.Sc., Assoc. M. Inst. C.E.

(Ordered by the Council to be published in Abstract form¹.)

THE intention of the Paper is to show (1) that the direct design of redundant frames may be facilitated by assuming that the total volume of material in the frame is a minimum; and (2) that in certain redundant frames it is possible to economize in material by the use of initial primary loading.

The volume of material in the frame (as expressed by $\sum AL$, where A denotes the cross-sectional area, and L the length of any member) is also a linear function of the "redundant reactions" and the external loads. This function will approach a lower limit as the redundant reactions approach limiting values, and at the same time the theoretical cross-sectional areas of certain bars will approach zero. Practical limits to the redundant forces are then set by the least sectional areas considered practicable for these members. The working stresses must, in a redundant frame without initial stresses, conform to the condition for minimum strain-energy, namely,

$$\Sigma f_k L/E = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where f denotes the working stress in any member, k the coefficient of influence of any redundant on such a member, and E the value of Young's modulus for such a member. Hence, in general, the ordinary limiting values of stress cannot be assigned to all the members, some of which will necessarily be uneconomically stressed. If there are n redundants, there will be n equations of the form (1). Professor A. J. S. Pippard, M. Inst. C.E., has described the operation of obtaining suitable simultaneous solutions of these equations by making as many of the stresses as possible equal to the limiting allowable stresses for the material.

If initial stresses are given to the frame, the strain-energy will be raised above the minimum, and there will be no arbitrary restriction upon the working stresses in the members. That is, all the members may be stressed up to their normal working limits. By assigning the working

¹ The MS. and illustrations may be seen in the Institution Library.—SEC. INST. C.E.

stresses in this manner, however, the initial deformation, Δ , of each redundant bar is arbitrarily fixed, since $\Sigma f k L / E = \Delta$, there being again n equations of this type. Any economy which may result from the self-stressing is a function of the ratio of the working compressive and tensile stresses; the investigation of composite frameworks of different materials is suggested.

Two frames are analysed in the Paper: a single-panel doubly-braced cantilever, and a five-panel cantilever with redundant bracings. Using working stresses of 3.5 tons per square inch in compression and 7 tons per square inch in tension, the economy effected is about 4 per cent. of the total volume in both these frames. Using working stresses of 4 tons per square inch in compression and 5 tons per square inch in tension, the single-panel frame gives a reduction in volume due to self-stressing of about 18 per cent.

The method of design by the criterion of minimum volume, where applicable, appears to lead at once to the most economical result without repeated trials, and it is by this criterion that the advantages of controlled initial primary stresses are judged.

In the examples considered, initial deformations of the redundant bars are required which are so small as to be difficult of realization in practice. The effect of relatively large initial deformations, however, which would reverse the direction of stress in a number of members, could perhaps be considered, but it would be difficult to ensure that the framework is stable when the live load is removed.

Initial primary stresses of such magnitude raise the strain-energy of the frame very considerably above the minimum; the Author has shown, however, that the "volume-function" of the frame is by no means identical with the "energy-function", so that to a certain extent it is possible to reduce the value of the "volume-function" while increasing the value of the "energy-function."

The Paper is accompanied by two diagrams.

Paper No. 5212.

“A Contribution to the Graphic Representation of
 $F(x_1x_2x_3x_4) = 0$.”

By SIMON CYTRYN, Assoc. M. Inst. C.E.

(Ordered by the Council to be published in abstract form¹.)

The graphic representation of a general function of four variables is usually possible only by means of a model, but cases are sometimes met with in practice when representation is possible with the aid of charts. The representation is either in Cartesian co-ordinates, nomographical or combined. Often nomographical representation is not possible and the usual representation in Cartesian co-ordinates is not convenient. The Author shows, in the Paper, that in the last case a suitable chart can be drawn.

The chart generally consists of a vertical line, a series of horizontal lines, and two series of curves drawn in the same quadrant. The readings of the chart is performed with the aid of a special scale. The two series of curves represent two variables, whilst the remaining two variables are read from the special scale.

As an example, a chart showing the relation between the effective depth, the bending moment, the concrete stress, the steel stress, and variable modular ratio in rectangular reinforced-concrete sections, has been drawn. The use of the chart is explained by means of several examples.

The Paper is accompanied by eight sheets of drawings.

¹ The MS. and illustrations may be seen in The Institution Library.—Sec. INST. C.E.

CORRESPONDENCE

ON PAPERS PUBLISHED IN

NOVEMBER 1938 JOURNAL.

Paper No. 5127.

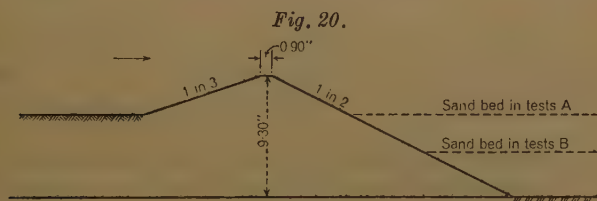
"The Protection of Dams, Weirs, and Sluices against Scour." †

By ROBERT VALENTINE BURNS, PH.D., B.Sc., Assoc. M. Inst. C.E.,
and

Assistant Professor CEDRIC MASEY WHITE, PH.D., B.Sc.

Correspondence.

Mr. Jack Allen observed that, since reading the Paper, he had carried out, with the aid of students in the Whitworth Engineering Laboratory of Manchester University, a series of similar tests on a model of a weir of quite different shape. He hoped that the results obtained might be of some interest. The weir (*Fig. 20*) was tested in a glass-sided flume



5.94 inches wide and 18 feet long. Its upstream face had a slope of 1 in 3, and its downstream face a slope of 1 in 2. The bed of the channel upstream of the weir was fixed at a depth of 3.0 inches below the crest of the weir; the experiments were performed with two initial levels of the downstream bed, namely, 3.0 and 6.0 inches below the weir-crest. The material adopted for the mobile downstream bed was a Leighton Buzzard sand similar to that used by the Authors. A control-notch at the downstream end of the flume enabled tests to be made with various tailwater levels, but in beginning any experiment water was first admitted to the lower part of the flume up to the crest of the weir, in order to prevent the artificial scour otherwise produced by the first rush of water from above

† Journal Inst. C.E., vol. 10 (1938-39), p. 23 (November 1938).

the weir. Time did not permit the exploration of different bed materials or of any tests of very long duration: runs of 30 minutes were made, and the bed near the toe of the weir had by that time attained approximated stability. In the subsequent description the following notation was adopted.

Tests designated A were made with a downstream bed initially 3.0 inches below weir-crest level.

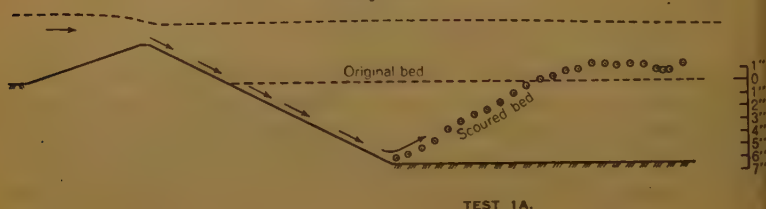
Tests designated B were made with a downstream bed initially 6.0 inches below weir-crest level.

H_1 denoted the head of water, in inches above weir-crest level, measured at a point 47.5 inches upstream of the weir-crest.

H_2 denoted the tailwater level, in inches, relative to the weir-crest level measured 43.5 inches downstream of the weir-crest.

Test 1A: no protective sill on the downstream face of the weir; $H_1 = 2.37$; $H_2 = 1.32$. As shown in *Fig. 21*, the bed was scoured deeply

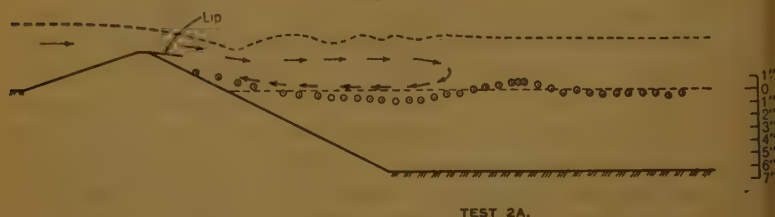
Fig. 21.



by the action of the main jet passing over the crest of the weir and clinging to its downstream face. The scoured material was deposited downstream where the bed, some 56 inches from the weir-crest, rose as much as 5 inches.

Test 2A: as a matter of interest, the effect was tried of attaching a lip to the crest of the weir, as shown in *Fig. 22*. That prevented the effective

Fig. 22.

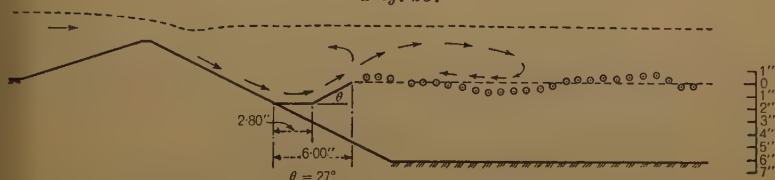


part of the stream from adhering to the weir-face, and resulted in a series of surface waves accompanied by a "reverse eddy", which actually transported sand some distance up the weir-face. The device was

however, subject to grave disadvantages : namely (a) it was very sensitive in action to the length and slope of the lip ; (b) the eddy and wave system generated was essentially unstable, and could be entirely changed by a slight variation of head ; and (c) the device could not operate successfully with lower tailwater levels or smaller discharges, because of the jet then dropping from its end, impinging on the weir-face and being directed at the sand-bed.

Test 3A : protective apron and sill as shown in *Fig. 23* ; $H_1 = 2.34$; $H_2 = 1.42$. The arrangement was found to work well.

Fig. 23.



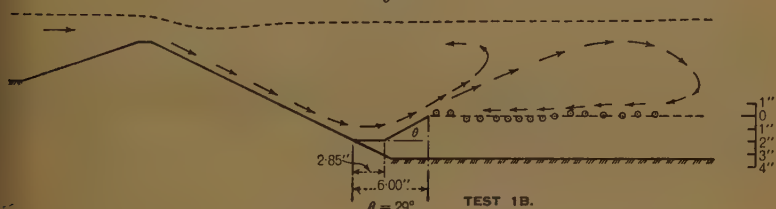
TEST 3A.

Test 4A : as in test 3A, but with $H_1 = 1.42$ and $H_2 = 0.38$. The sill was again satisfactory.

Test 5A : as in test 3A, but with $H_1 = 0.57$ and $H_2 = -1.11$. No noticeable movement of the sand-bed occurred.

Test 1B : with protective apron and sill as shown in *Fig. 24* ; $H_1 = 2.36$;

Fig. 24.



TEST 1B.

$H_2 = 1.54$. The general bed-movement was slight, but was back towards the weir for a distance of some 20 inches downstream of the sill.

Test 2B : as in test 1B, but with $H_1 = 1.33$ and $H_2 = 0.31$. The sill was satisfactory, very little movement of sand-bed occurring anywhere.

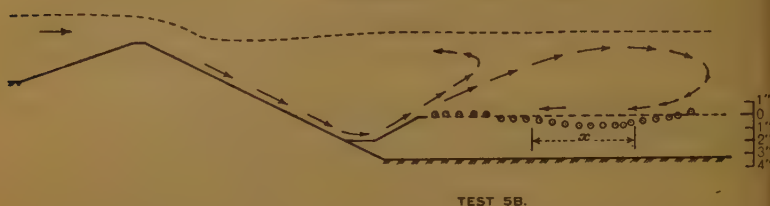
Test 3B : as in test 1B, but with $H_1 = 0.61$ and $H_2 = -0.98$. There was scarcely any movement of the bed.

Test 4B : as in test 1B, but with $H_1 = 1.40$ and $H_2 = -0.73$. A slight movement of sand-bed occurred back towards the weir for a distance of some 12 inches downstream of the sill.

Test 5B : as in test 1B, but with $H_1 = 2.38$ and $H_2 = 0.56$. Over the length x in *Fig. 25* (p. 254), the sand-bed was from 1 to $1\frac{1}{2}$ inch higher if

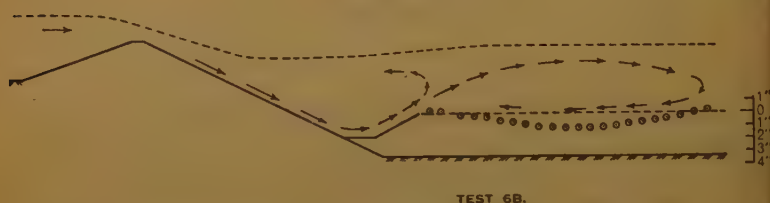
it were measured in the centre instead of at the side of the flume. That effect was due to transverse currents.

Fig. 25.



Test 6B: as in test 1B, but with $H_1 = 2.27$ and $H_2 = -0.75$. The effect was shown in Fig. 26.

Fig. 26.

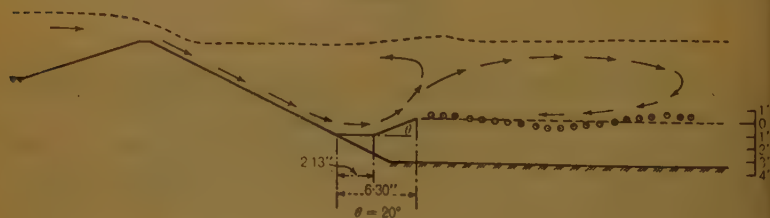


Test 7B: as in test 1B, but with $H_1 = 1.43$ and $H_2 = -1.74$. The movement was as in test 6B, but was not so marked.

Test 8B: as in test 1B, but with $H_1 = 0.54$ and $H_2 = -3.17$. No appreciable movement of sand-bed occurred.

Several experiments were next made with a different design of sill (Fig. 27), and with a sand-bed initially 6.0 inches below the weir-crest.

Fig. 27.



On the whole, that sill did not behave so well as the other design. An interesting phenomenon was observed when $H_1 = 2.39$ and $H_2 = 0.40$. For a time material was scoured from the bed and carried by the reverse current (indicated in Fig. 27) upstream to the sill, where it formed a bank. That bank reached such a height as to alter the regime of the flow, so

that a lee eddy was produced and the bank moved downstream in the form of a ripple, leaving the end of the sill exposed. After a short time, the original eddy-system re-appeared and again carried material back to the sill, when the whole process was repeated.

To summarize, the Authors had found that a sill with slope of 20 degrees was best, with their weir, "to cope with a wide range of flows and depths." In Mr. Allen's tests on an entirely different shape of weir, an angle of 20 degrees also behaved reasonably well, although not so satisfactorily, for all depths and discharges explored, as a 27-degree slope. It was quite possible, however, that a different length of apron would have favoured the smaller angle. Incidentally, the angle of 27 degrees was very nearly that assumed by the sand-bed in test 1A when no protective device was employed.

Mr. W. E. Doran observed that *Fig. 4* (p. 29 §) showed that the final bed form in such models was independent of the grain-size of the bed-material, within the limits set by the requirements for turbulent flow on the one hand and the transportability of the bed-material by the hydraulic forces in the model on the other. With regard to the influence of grain-size on the rate of scour, however, the Authors seemed to infer rather more than the facts presented would warrant. It did not seem valid to conclude, from an experiment with only two sizes of sand, where three out of five results showed time-ratios proportional to the grain-size raised to the power $3/2$, that $t/k^{3/2} = \text{constant}$ was a general law. The Authors' attempt to test the validity of that hypothesis by applying the principle to two models of different scale-ratio was ingenious, but inconclusive. To test the relationship of grain-size to rate of scour a more comprehensive series of experiments would be necessary. It was generally recognized that the theoretical time-scale could not be applied to rates of scour in models, and it would appear that further research on that matter would be very desirable.

The actual rate of scour was not usually of much importance in designing protective works, quantitative results being generally sought. It was, however, of considerable importance in tidal models where the amount of bed-movement in each tide had to correspond, at least approximately, to natural effects if reliable results were to be obtained; in such cases a method of determining grain-size would be of great use.

The Authors had presented their results as showing the effect of the angle of the sill upon its action under various tailwater depths (*Fig. 11*, p. 38 §).

It was noted that the critical angles given in *Fig. 11* had been obtained by raising or lowering a hinged flap. It was evident that in altering the angle of such a sill the height of the sill was correspondingly altered. The

§ Page numbers so marked refer to the Paper, (Journal Inst. C.E., vol. 10 (1938-9), p. 23 (November 1939).)—SEC. INST. C.E.

height of the sill, apart from its angle, had a very great influence on its effect, and therefore, since both the height and the angle were altered, the curves in *Figs. 11* and *12* (pp. 38, 39 §) did not really express the relationship between the sill-angle and the tailwater depth. What they did show was the effect of the angle of a hinged flap of length $0.124P$, an effect which was not necessarily true of flaps of different lengths, and therefore not necessarily generally valid.

Fig. 14 (p. 40 §) gave a comparison between a "long" and a "short" sill, and it was found, for example, that when $T = 0.543P$ the optimum angle of the shorter sill was 33 degrees and that of the longer sill 23 degrees. Under those circumstances the "long" sill would be nearly twice the height of the "short" sill, and much larger in volume. Whilst it was obvious that the angle and length determined the height of the sill, it was difficult to visualize a sill in terms of angle of slope and length of hypotenuse. It would be natural to expect that the shorter the sill the more steeply it would have to be turned up to produce the most effect. Mr. Doran suggested, therefore, that to determine the effect of different angles of sill the height should have been kept constant, whilst a further series of experiments would then have been required at constant angle and different heights.

Comparison of *Figs. 13* and *14* (p. 40 §) gave somewhat puzzling results. Although the optimum angle in *Fig. 13* was not shown, the curves in *Figs. 13* and *14* for the same length of sill appeared to differ very much in slope.

In the bottom diagram in *Figs. 10* (p. 36 §) showing the hinged sills, all three were shown as hinged about the same point, namely the end of the apron. If that method were used in obtaining the comparison then a further variable was introduced, in that the length of the apron was altered. The length of the apron in the 21-inch model was given as 4.8 inches and the lengths of the two sills used in *Fig. 14* as $0.19P$ and $0.071P$, or 3.99 inches and 1.49 inch respectively. The longer sill was therefore no less than 0.83 times, and the shorter sill 0.31 times, the apron-length, and, as the longer sill was found to work better at a flatter angle than the shorter one, that further accentuated the effect of the difference in the apron-length.

Possibly the Authors had investigated the influence of the sill-height also, and if so their results would be of interest.

It was stated that the results obtained could be readily applied to sluice-gates, but it was not clear how the ratios h/P and T/P could be applied to such structures.

It would be of the greatest value if a series of formulas or graphs could be obtained from which the best height and slope of sill could be obtained once the conditions were known, but the Authors did not appear to have

succeeded in deducing any general laws as a result of their experiments. The various graphs shown represented the results of different experiments under different conditions, rather than an attempt to investigate separately the different variables involved by so arranging the experiments that only one variable was altered at a time. The angle and height of the sill varied simultaneously, for instance, in *Fig. 11* (p. 38 §). In *Fig. 12* (p. 39 §) the length of the sill was slightly different to that in *Fig. 11*, whilst in *Fig. 14* (p. 40 §) it was different from either. The sills in *Figs. 17* (p. 44 §) seemed to be different in height from those in other experiments. The result was that it was really impossible to arrive at any conclusion of a basic character by a comparison of the results obtained.

In *Figs. 15* (p. 42 §) a comparison was made between a Rehbock sill and a sloping sill which was varied in angle to suit the water-levels. The larger sloping sill was obviously much bigger than the Rehbock sill, and although the Authors mentioned that the smaller sill was approximately the same size as the Rehbock sill, it appeared from *Figs. 15* (particularly the top three diagrams) to be much higher than the Rehbock sill. In actual practice it was usually necessary to contend with a considerable range of water-levels, and it was interesting to note that in such cases the Authors had found a 20-degree sill to be as good as the Rehbock type. Particulars of those experiments would be of interest.

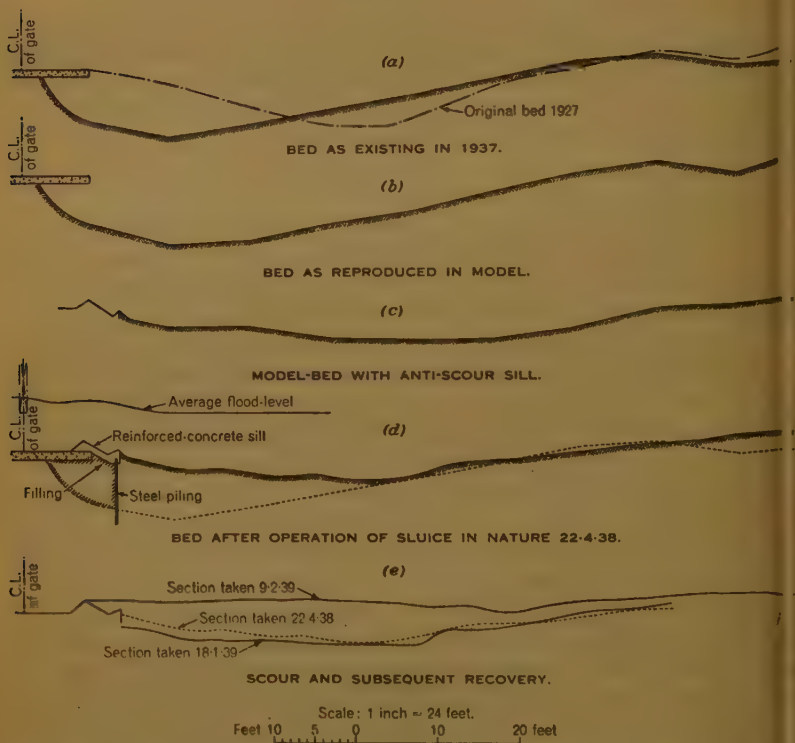
In *Figs. 17* (p. 44 §) in which three of the four sills shown appeared to be of approximately the same height, and therefore comparable, it was shown that the $26\frac{1}{2}$ -degree sill failed when $T = 0.5P$ approximately, the ratio h/P being 0.205, whilst the $12\frac{1}{2}$ -degree sill gave adequate protection; yet the curve in *Fig. 12* (p. 39 §) gave the optimum angle as 25 degrees for $T = 0.5P$ and $h/P = 0.175$ or over. Those results appeared to be entirely conflicting. Probably the sills were of different height in the two cases, and it would be of interest to know if that were the case. If so, what were the relative heights above the apron? If that were the explanation of the difference, then the Authors' conclusion on pp. 44-45 § that "if the sill has to cope with a wide range of flows and depths the best angle seems to be 20 degrees," which apparently was based upon the experiments shown in *Figs. 17*, might be found to be no longer valid for a sill having a height different to the one at which the tests were carried out. That served to emphasize Mr. Doran's view that the Authors were not entirely correct in presenting the results of their experiments as the influence of the sill-angle on the behaviour of the sill under various conditions, and hence emphasized the danger of making general deductions from the results presented.

Mr. Doran fully agreed with the Authors regarding the importance of placing the apron at a sufficient depth below the lowest tailwater level. Scour-holes of surprising size had frequently been found below com-

paratively small sluice-gates, due principally to the neglect of that precaution. Such cases were a difficult problem. The apron itself was nearly always undermined, and it was useless to fill in the scour-hole since unless other measures were taken, the filling would be scoured away again.

He had carried out some experiments on a structure of that kind early in 1938. The structure was a sluice of 18 feet span, and 6 feet high. The apron was much too short, being only 8 feet 6 inches in length, and

Figs. 28.



was at too high a level. A scour-hole 9 feet deep was formed below it and it was possible for a diver to walk upright underneath the apron.

A model was constructed and the bed was modelled to the conditions existing when the structure was built. The model was then run under water-levels obtained from past records, and was found to reproduce with great accuracy the actual scour obtained in nature (*Figs. 28 (a) and (b)*). Further experiments showed that, whilst satisfactory protection could be obtained with a triangular sill at high tailwater levels, scour occurred when the levels fell below a certain height. Consideration of the problem

suggested that at the lower levels the sill was acting like a miniature weir, the water falling over its vertical face and scouring the bed.

That suggested the possibility of remedial action by extending the sill with a slope in a downstream direction, ending in a small triangular sill. That was very successful, and the secondary sill was found to prevent any scour even when the downstream level was only slightly above the top of the primary sill. The constructional work required was also very simple. The model-experiments showed that to enable the sill to work properly, streamline wing-walls would be required on either side.

The work was carried out in accordance with the indications of the model-experiments, and when it was completed a quantity of shingle from the bar formed by the erosion was dumped into the scour-hole. The sluice-gate was then opened and it was found that in a very short time the dumped material had been levelled off and pulled up towards the structure by the action of the sill. The final line of the bed agreed very closely with the result obtained in the model, as might be seen on reference to *Figs. 28 (c) and (d)*.

Towards the end of 1938 some workmen who wished to carry out some minor bank repairs opened the sluice and left it open at levels at which it was not intended to be used and at which model-experiments had shown that scour would take place. That action resulted in considerable scour in quite a short time. When that was discovered the bed was found to be as shown in *Figs. 28 (e)*. It was decided to wait until higher levels were available and then to operate the gate to see whether or not the sill would pull back the bed-material. The gate was left full open during the floods which occurred at the end of January, and when surveyed on the 9th February, 1939, the scour-hole was found to be completely filled in and the bed-material piled up to the top of the primary sill, as shown in *Figs. 28 (e)*. Since there was not sufficient material downstream of the sluice to fill in the scour-hole, it was evident that it had been filled in by material carried down by the river during the flood. The scour-hole was filled in as shown on the section during a period of only 10 days, some 350 cubic yards of material having been deposited during that time. The material filling the hole was sand. That result showed that it was not necessary to have filled in the hole by dumping from barges, as the sill would have done that if left to itself.

Mr. Doran mentioned that instance because the use of a double sill of that type was quite new, as far as he was aware, and because it afforded a method of dealing with existing structures where the apron had been fixed at too high a level and had become undermined in consequence, a condition very common at mills and sluices on English rivers. It also showed a very important characteristic of triangular sills, such as those described by the Authors, as compared with "energy-killing" devices such as baffle-piers; namely, what might be called the recuperative effect of the triangular sill, in being able to replace material which had been

scoured out by misuse of the sluice-gate. It might happen in some cases that for some unforeseen reason the sluice might be opened at water-levels at which it was never intended to be operated, with the result that damage might be done in a few hours which it would take weeks to repair if it were not the property of the sill to produce a "piling eddy" downstream of the structure. If the river carried any appreciable amount of material in times of flood it would not be necessary in many cases to go to the expense of filling in the scour-hole, since that would be done, as in the instance just given, by the sill itself. All that was required was a suitable sill and some protective piling to keep the apron secure until the scour-hole had become filled in.

The Authors in their investigation had necessarily to confine themselves to an ideal structure discharging between the parallel walls of the flume. In actual structures, however, one of the principal difficulties which arose was with the arrangement of the structure in plan so that a gradual enlargement could be obtained, without which vertical eddies would form that might destroy completely the action of the sill. The importance of that point did not appear to be sufficiently realized by engineers. In designing a structure it should receive very careful consideration, and in running model-experiments the greatest care should be taken to ensure that the conditions of the entrance and exit of the water in relation to the structure were as nearly as possible those of the prototype. Experience with models showed how difficult it was to foresee the effect of apparently small variations between approximately similar structures, and emphasized the necessity of making a careful model-experiment with every structure before deciding upon the best height and slope of sill and its auxiliary protective works. Nothing could be more dangerous than to decide blindly upon a particular sill and to adopt it as a means of protection without full knowledge of its action under the conditions existing in the case under consideration.

Apart from the intrinsic interest of the Paper, it was of value in indicating further useful lines of investigation in connexion with anti-scour sill design.

Dr. Ing. Theodor Rehbock, of Baden-Baden, observed that the investigations of the Authors could be divided into three parts: firstly, the examination of the general rules of hydraulic-model research, especially in regard to the laws of similarity, the experimental equipment used, and the choice of the bed-material of the model; secondly, the examination of the scour downstream of a weir provided with a horizontal apron, and having end sills of different shape; and thirdly, the comparison of the results obtained with triangular sills with those of the "dentated sill" and of the "Osage stepped sill."

The first part of the Paper gave a clear summary of the most important fundamental rules of hydraulic research, and merited special attention. Dr. Rehbock fully agreed with the Author regarding the importance of using models of a size as small as possible for the tests; that was to say,

of the smallest size with which reliable results could be obtained, depending on the laws of similarity. For many years he had advocated the expediency of small models on practical considerations, against the widespread opinion that large-size models were more advantageous, an opinion especially supported by American engineers, including the late Mr. John R. Freeman, the enthusiastic promotor of hydraulic research all over the world, who was of the opinion that large-size models would give better results.

He also agreed with the Authors that the stable final form of the model-bed was, within wide limits, much the same with different sizes of bed-material. The model-tests executed with sand and gravel of different diameters at Karlsruhe had repeatedly shown that to be true, as could be seen, for instance, in his contribution to the English edition of Mr. Freeman's book*. *Fig. 199* on p. 203 of that book showed such a comparison for bed-material of different sizes for the depth of scour on bridge-piers, and *Fig. 222* on p. 228 for the excavations downstream of the apron to the Ryburg-Schworstadt weir on the Upper Rhine. The conclusions arrived at from those tests agreed with those of the Authors, in that the final excavations were, contrary to expectations, rather deeper with a coarser sand than with a finer sand. The time in which the formation of the excavation was completed and the maximum depth of scour was attained was certainly longer with the coarser bed-material.

The Authors gave in Table I (p. 30 §) a series of figures which showed that with sand of 3.3 times larger diameter, the time necessary to finish the excavation was increased 6 times, and that to produce half, or any other fraction of the maximum scour-depth, the duration of the tests had also to be increased 6 times. According to those observations, therefore,

$$\propto = \frac{\text{time-relation}}{\text{grain-size relation}} = 1.82.$$

Nearly the same value was observed at Karlsruhe with Dr. Rehbock's tests for the termination of the scour-depth at bridge-piers.

The evidence that the grain-size of the bed-material could be changed within wide limits without influencing perceptibly the definite form and the depth of the excavation was of value, since the duration and the costs of model-tests could be appreciably diminished by the choice of a finer grain of bed-material. The duration of the tests for the Ryburg-Schworstadt weir, for instance, could be diminished by one-thirty-sixth (that was to say, from 24 hours to 40 minutes), by replacing the fine gravel of 9 millimetres diameter, used in the beginning, by sand of a mean grain-diameter of 0.75 millimetre. By that modification it became possible to execute many more tests in the time available. That pro-

* "Hydraulic Laboratory Practice", edited by John R. Freeman. New York, 1929.

§ *Ibid.*

cedure was restricted by the fact that the similarity of the excavations would be lost if the mean diameter of the particles forming the model bed were to become sensibly smaller than 0.5 millimetre.

The discharge over the 21-inch-high weir-model with varying head measured by the Authors and shown in *Fig. 3* (p. 28 §) agreed very well with the values of μ in Dr. Rehbock's formula for weirs with a circular cylindrical crest *, namely

$$\mu = 0.312 + \sqrt{0.3 - 0.01 \left(5 - \frac{h}{R_m} \right)^2} + 0.09 \frac{h}{P},$$

if R_m were replaced by R_1 instead of by $\left(\frac{2R_1 \cdot R_2}{R_1 + R_2} \right)$, as had been done by the Authors. The insertion of R_1 seemed suitable, because R_2 , beginning only at the crest-line of the weir, would have no perceptible influence on the quantity of discharge.

In the second part of the Paper the Authors referred to the well-known fact that a horizontal apron without an end sill, even if it were of a considerable extent in the direction of the flow, could not prevent dangerous excavations near the end of the apron.

The Authors commented also upon the great influence of the height of the tailwater level over the apron on the scour. They showed that influence in *Figs. 9* (p. 35 §) by the measured vertical sections of the excavations produced with different heights of the tailwater above the apron. Those lines showed that a very low, as well as a very high, position of the tailwater produced deep excavations, whilst intermediate positions of the tailwater level produced much less scour, which might disappear even completely in some cases at the apron itself. It was therefore necessary to avoid too high, as well as too low, a level of the tailwater above the height of the apron. That phenomenon could only be interpreted by taking into consideration the different ways in which the flow of the water might be influenced by the difference between the height of the tailwater level and that of the apron for certain discharges.

In most cases it was impossible to change the height of the tailwater because that depended only on the quantity of discharge, and on the form and the slope of the river-bed downstream of the weir. The height of the apron had therefore to be chosen in such a manner that the tailwater depth above the apron became neither too great nor too small. The proper choice of the height of the apron, which up to the present had been impossible to fix accurately without model-tests, was of the greatest importance, since most accidents with weirs had been produced by a faulty position of the apron. It was not possible to consider that problem exhaustively in the present discussion, and therefore only a few

* Handbuch der Ingenieur-Wissenschaften, vol. 2 (Part 3, section 1), p. 54. Leipzig 1912.

§ *Ibid.*

comments would be made to show how the height of the apron had to be determined.

The apron had to lie at such a height that with all possible discharges a diving water stream with a surface-roller was produced, for then the water would engender a shallow trough-shaped excavation, ending upstream near the foot of the apron. Out of such a shallow basin the water would flow calmly and equally distributed in the river-bed downstream. That most suitable, and therefore desirable, manner of flow was formed with a certain discharge on a given weir and apron only with a tailwater level lying between two fixed limiting heights.

When the tailwater level lay lower than the lesser of those two limiting heights, the water stream, which passed the apron in shooting flow, would not submerge immediately at the end of the apron in the tailwater. It was first deflected on high, forming a remarkably high standing wave that might be called a "spring-wave", at the end of which the water stream dived in a steep track to the bottom, where it formed deep excavations which reached far downstream and increased the possibility of washing away the banks to a considerable extent.

If, on the contrary, the tailwater level reached or exceeded the upper limiting height, that was to say, lay too high above the apron, the water stream, in changing from the diving flow with surface-roller to the flow with waved surface, would form—often only for a short time—a very dangerous transitional state which might be termed the "stretched flow." The "stretched flow" was formed if the water stream, although it was submerging above the apron under a surface-roller, did not reach the apron. It could not, therefore, be deflected by the apron in the horizontal direction. It was flowing rather with so flat a slope over a strongly prolonged ground roller, that it struck upon the unprotected river-bed downstream of the end of the apron. There it would produce deep excavations, which extended in most cases upstream directly to the apron in considerable depth, undermining the foundation of the apron. That form of flow seemed to Dr. Rehbock to be the most dangerous of all for a weir. Certainly it had caused the destruction of many weirs, although in most cases the real reason of the damage might not have been understood.

The Authors tried to interpret the excavations observed with their pests by the position of the water jump. That, however, seemed to be impossible, for, whether upstream or downstream from a water jump¹, formed on a horizontal bed, the water stream would not flow downwards in such a direction as was necessary to produce deep excavations. Such a downward-directed water stream occurred according to the foregoing

¹ In the footnote of p. 45 of the Paper, Dr. Rehbock's formula for the water-depth T downstream of a water jump was not given correctly. The value T had to be calculated from the depth t_0 and the velocity-head H of the shooting current upstream of the jump, the correct formula being $T = t_0(2\sqrt{H/t_0} - 0.45)$.

declaration as a result of too high or too low a tailwater level, and the downward-directed water striking upon the unprotected river-bed seemed to be the real cause of the dangerous deep excavations downstream of a weir.

The Authors, having observed that a horizontal apron alone could not prevent dangerous excavations, were of the opinion that it was necessary to use "indirect methods of protection" of the river-bed, and suggested either energy-dissipators or flow-deflectors. Energy-dissipators, formed by a series of steps, blocks, piers, or arrows, built on the face of the weir or on the apron, had been shown to be of little advantage, and the Authors accordingly examined more thoroughly flow-deflectors in the form of sills placed at the extreme downstream end of the horizontal apron.

The sills used by the Authors were mostly of considerable size, lifting the water stream from the bed for a long distance, and creating between the river-bed and the water stream a ground roller of remarkable dimensions with an upstream-directed current above the bottom.

A heavy silting-up of the river-bed by that current was the result. It was formed by bed-material whirled up farther downstream, where the water stream struck the river-bed. At that place, and downstream of it, extensive excavations took place for a considerable distance.

The Authors did not express clearly what they had in view during their experiments, but it seemed that they wished to produce a strong building-up of the river-bed immediately downstream of the apron of the weir, since all the contour lines of the river bottom shown in *Figs. 15 and 16* (pp. 42-43 §) after tests with the specially recommended triangular end sills showed a heavy sedimentation above the height of the apron. Contrary to the opinion of the Authors, Dr. Rehbock considered such a high sedimentation of the river-bed downstream of the apron to be detrimental rather than advantageous. It should be avoided as it was not necessary for the protection of the weir and its apron, and gave rise to the destruction of the river-bed downstream of the weir. A well-constructed weir should calm the water flow and should introduce it, equally distributed, in a short stream-length in the river-bed adjacent, and should then re-create the normal flow as soon as possible; by doing so it would reduce the length of bank which had to be protected.

To satisfy that requirement, it was necessary to create close to the apron a shallow basin extending below the height of the apron, but not to raise up the river-bed above the height of the apron, since an increase in height would only shift the excavation downstream, and would produce an increased slope of the river-bed below, on which shooting flow might arise; that would lead to the formation of washed-out channels through which the discharge was locally increased, thus preventing the equal distribution of the water in the river-bed.

Most of the sills examined by the Authors were simple triangular sills whose vertical side was directed downstream. The Authors, attaching special value to the determination of the best angle of the inclined surface, had come to the conclusion that the best slope varied, within wide limits, with the quantity of the discharge and with the tailwater level, even with the same weir. The Authors recommended a moderate slope-angle of 20 degrees, that was to say, a slope of 1 in 2.75, if the quantity of discharge and the tailwater depth varied considerably, as was generally the case with weirs. In 1921 Dr. Rehbock had made tests with a view to finding, by means of such triangular sills, an arrangement which would avoid dangerous excavations. For various reasons he had soon to abandon that form of sill, amongst them being the fact that the same triangular sill could not be adapted satisfactorily to changing discharges; further, he had succeeded meanwhile in finding the dentated sill, which showed itself superior to other known sills in various ways. Concerning the comparison of the triangular sill with the dentated sill in the third part of the Paper, he wished to point out that in none of the five sections of *Figs. 15* (p. 42 §), measured on the apron with a dentated sill by the Authors, was any scour observed near the apron, and that in none of the sections was the slope-line of the excavation inclined more than 1 in 3.

Those facts showed that with respect to the protection of the apron the dentated sill had met all requirements, as had also been the case with the triangular sill. In the tests with a dentated sill it should have been possible even to have reduced the excavation somewhat more, thereby shortening the length of scour by reducing the height h_z of the dentated sill; that height should, according to his formula of 1931, be not more than :

$$h_z = 0.08 h^{2/3} \cdot P^{1/3} \quad . \quad . \quad . \quad . \quad . \quad . \quad (i)$$

With the head $h = 0.175P$, which had been adopted in the Authors' tests, the height of the dentated sill had to be, according to that formula, not more than $h = 0.025P$, or 13 per cent. and 36 per cent. respectively of the height of the two triangular sills used by the Authors in their comparison with the dentated sill shown in *Figs. 15* (p. 42 §), which had heights of $0.19P$ and $0.07P$ respectively. According to (i), with the head $h = 0.175P$ and with a weir-height P of 40 feet the dentated sill should be only 1 foot high, and with a weir-height of 80 feet it should be only 2 feet high. That small height of the dentated sill reduced the quantity of material necessary for the construction of the two triangular sills used by the Authors to 1.2 and 8.8 per cent., and diminished the danger of their being struck by floating trunks passing over the weir.

The Authors on p. 36 § stated that "the problem [of protection] finally resolved itself into one of finding the smallest possible apron and deflector

compatible with effective protection, the aim being to minimize both first cost and maintenance."

It seemed to him that the dentated sill, giving effective protection and reducing the material necessary for the flow-deflector to $1/83$ or $1/11$ in the two sizes of that necessary for the triangular sill (depending on which sill was used), should be given preference, the apron requiring the same quantity of material with the different sills.

Further important tasks of the end sill of an apron were the protection of the river-banks by calming the water, after the fall over the weir, as quickly as possible; the leading-off of the shingle and floating material without retardation; and the prevention of low-water excavations, which might be produced on account of insufficient tailwater depth during times of low water, with sills having a vertical downstream face, as had the triangular sill. None of those requirements, all of which were satisfied by the dentated sill, was mentioned in the Paper, whereas the Authors asserted that a triangular sill with an angle of 20 degrees, when tested, was "as good as Rehbock's, whilst it had the advantage of being free from constructional irregularities likely to require maintenance." To that statement Dr. Rehbock replied that the dentated sill was actually of very simple construction and of a shape which all requirements conformed excellently to. Even the hardest shingle in torrents would do no damage to the teeth if they were properly constructed.

That had been confirmed again, recently, by the experience gained on the weir at Pizangon, on the Isère torrent in the French Alps, which had a maximum discharge of 53,000 cusecs, and a fall of 46.5 feet. That weir, completed in 1931, had a dentated granite sill 2 feet high. Careful examination of the teeth after 7 years' use showed that the cross section had only been reduced by wear to the extent of $1/750$ part by the very hard shingle of quartzite, granite, and gneiss carried by the Isère. Similar observations had been made on other weirs with dentated sills, even in large mountain rivers. He had not been advised, up to the present, that any dentated sill had had to be repaired or renewed. He could not, therefore, accept as being fair the opinion of the Authors that the dentated sill required special maintenance. From the practical experience of the last 15 years, during which period hundreds of dentated sills had been installed in more than thirty different countries, including some on small weirs in England, it had been proved that such maintenance was not necessary.

Many thousands of tests had been carried out at Karlsruhe with dentated sills on hundreds of weirs of different shapes, the observed results on many of the models used having been compared later with those obtained on the actual weirs when they were constructed. Apart from the work in the laboratory at Karlsruhe, other experimenters had repeatedly tried to find some other arrangement superior, or at least equal, in efficiency to the dentated sill, but without success. Although there were more weirs

and dams in British India than in any other country, it was surprising that it was one of the few countries where the dentated sill was not used. That might be because the comparative model-tests with dentated sills, which for several years had been carried out in the Punjab Irrigation Research Institute in Lahore, had not been performed correctly; the dentated sill had not been placed in the correct spot near the unprotected river-bed, but at a distance of 60 feet upstream from the end of a layer of concrete blocks forming the downstream part of the apron, a site where it could be of no real use. The results were therefore unsatisfactory, but they had been distributed throughout India by the official publications of the Central Board of Irrigation, and also by the Reports of the Punjab Engineering Congress in Lahore. The publication of those incorrect results had undoubtedly prevented Indian engineers from adopting, and even from making trials with, the dentated sill. All Dr. Rehbock's attempts to correct the unfavourable effect of those incorrect experiments, by carrying out model-tests at Karlsruhe for Indian weirs—for example, for the Marala weir reconstruction—had failed, probably because many Indian engineers were unaware of the criticisms that he had made of the tests in India. The damage caused by not using dentated sills was bound to be considerable, in view of the losses calculated on some single weirs for which special inquiries had been made. For example, at the Marala weir the expense incurred in lowering a layer of concrete blocks, 60 feet broad by 4,000 feet long, through a distance of 4 feet, could have been avoided by the installation of dentated sills, as his model-tests had clearly shown.

He could not discuss so completely the comparison made by the Authors between triangular sill and the Osage sill, because the latter was not sufficiently well known to him. Since the Osage sill was merely a triangular sill with low steps, the effects of the two sills could not be very different. Nevertheless, the scour-lines observed by the Authors showed that the Osage sill gave better results in the tests, as it did not produce the excessive height of sedimentation downstream of the apron that was created by the triangular sill. The Osage sill, however, as it was continuous like the simple triangular sill, would not exert a beneficial influence on the river-bed downstream of the apron, as did the dentated sill. A continuous sill could not be adapted so well as an interrupted sill, to widely different discharges, and, having a high vertical fall downstream, it produced low-water excavations immediately beyond the apron; in some cases that would necessitate a secondary apron if the sill were high.

Finally, the fact might be stressed that the Authors' tests had been restricted to two-dimensional flow arising on weirs with an equally divided discharge over the whole length of the weir, such as would arise in the case of a fixed weir without a spillway. The tests took no account of three-dimensional flow over fixed weirs with spillways, and through sluice-gates in dams; in such cases cross currents, produced by the unequal distribution

of the discharge, might appear, and might have an important influence on scour.

The Paper contained valuable general remarks on the fundamental laws of hydraulic research, and the results of carefully executed model-tests, but the conclusions drawn from the tests carried out seemed to Dr. Rehbock to be erroneous, and did not agree with his experience, gained on observations of actual structures and on models. He was afraid that the deductions derived from the Authors' tests would not serve to dissipate the want of confidence in the value of the dentated sill that had been created in the minds of the British engineers through the wrongly-carried-out model-tests at Lahore. That was a serious matter in view of the fact that the dentated sill had proved its value all over the world. The triangular sill, the predecessor of the dentated sill in his experiments, could not be regarded as being of the same value.

The principal difference between the Authors' opinion and his own seemed to lie in the supposition of the Authors that it was desirable to obtain as high a level as possible of the river-bed immediately downstream of the apron, whereas in his opinion the best solution was to form a shallow basin downstream of the apron, ending upstream immediately at the apron. In that basin, in most cases excavated by the water itself, a ground roller had to be formed with an upstream-directed flow, just above the bottom, which prevented the particles of the river bed immediately downstream of the apron from being carried away downstream. In that case, no scour could occur immediately at the end of the apron, and thereby endanger the apron and even the weir, as well as increase the possibility of piping.

The Authors, in reply, wrote that Mr. Allen's experiments, when compared with their own, showed that considerable change in the downstream slope of the weir-face had but little effect on the action at the toe. A few feeler experiments had also led them to that conclusion. Among other things, the combined thickness of the overflowing jet and associated mixing zone just upstream from the sill influenced its action. It was the angle of slope of the sill combined with the length of the slope in the direction of flow which had the greatest effect on the protective value of the sloping sill. The Authors' experiments had disclosed the danger-region *D* in *Fig. 11* (p. 38 §), and their suggested 20-degree sill-slope was a compromise to avoid the danger-region and other troubles, and was less than what at first appeared to be the optimum angle. They thought that those considerations would together account for a little more than the extra 7 degrees mentioned by Mr. Allen. They did not think that the shape of any toe should be based on the shape of an unprotected bed, for in their own experiments with completely exposed beds, they had found much steeper slopes than that mentioned by Mr. Allen.

The natural bed adjusted itself until the drag was approximately uniform, whereas if an artificial apron and sill were so designed there was risk of premature breakaway when conditions changed. It seemed best to design, if possible, for increasing drag, right up to the point where breakaway was desired, as there was then less risk of negative drag near the toe.

As Mr. Doran pointed out, the rate of scour relation should on no account be regarded as an established generalization applicable to all kinds of bed-movement problem. It did not, for example, apply to wave-action on a sea-shore, for there an entirely different motion occurred, with the paradoxical result that heavy shingle was moved more rapidly than sand. It was, however, interesting to hear that Dr. Rehbock had observed that the rate of scour near bridge-piers varied approximately in the same manner as with the Authors' weirs. Perhaps in the near future it would be possible to explain such observations by reference to existing theories of turbulent suspension.

The Authors wished to point out that the series of bed formations in the various Figures in the Paper were selected from many hundreds to illustrate either particular sequences of actions or particular characteristics of certain sills, and although quite typical they were not the main experiments on which *Figs. 11 to 14* (pp. 38 § *et seq.*) were based. Some differences were therefore to be expected, but that to which Mr. Doran referred was not an experimental inconsistency but was due to comparing sills which were of different lengths. Mr. Doran should compare *Figs. 17* (p. 44 §), not with *Fig. 12*, but with *Fig. 14*, which concerned a rather similar length of sill; he would then find satisfactory agreement.

The Authors had certainly varied more than one variable at once, but Mr. Doran's criticism that they should not have done so seemed without foundation. Whenever the effect of change of geometrical shape was being investigated it was, in general, fundamentally impossible to change one dimension only. Further, Mr. Doran could not avoid the difficulty by using rectangular co-ordinates in place of the polar ones used by the Authors. The height of the sill could certainly be used as one of the variables, but the Authors avoided its use as they had found that it was at times misleading. The height was readily found from the length and angle of the sill, and those two variables had the advantage that they drew attention to the immediate purpose of the sill, which was to deflect the stream through a certain angle, and they also enabled a very wide range of complicated phenomena to be co-ordinated and condensed into the simple diagrams, *Figs. 11 to 14* (pp. 38 § *et seq.*). Dr. Rehbock's criticism that the depth of scour was a poor measure of the performance of a sill was no doubt true, but the Authors viewed the matter from a different standpoint. They also had found that a moderate depth of scour greatly assisted the action of most sills, but they considered that a sill

which required such assistance had less margin of safety than one which would act without it. If that were so, then their use of the depth of scour seemed justified, particularly when, as in the present case, it was considered in relation to its distance away.

The Authors would be very sorry indeed if any remarks of theirs seemed to disparage the dentated sill. It was one of the finest examples of successful scientific design which it was possible to find. In the light of Dr. Rehbock's experience they unreservedly withdrew the criticism implied in their remark "as good as Rehbock's, whilst . . . free from constructional irregularities likely to require maintenance." In writing that, they had had in mind the point that the simpler the form the better. In India the dentated sill had had a bad Press, due to failure to appreciate what its purpose was. At Lahore experiments had previously been made with dissipators, and from the account of the experiments it seemed clear that an attempt had been made to use the dentated sill also as a dissipator, whereas, of course, it was essentially a deflector, and a very successful one.

In comparing the relative sizes of sills, possibly Dr. Rehbock had misread *S* as the height of the sill, whereas it was actually the sloping length. The main comparisons between the dentated sill and the simple sloping sill had been made with sills approximately equal in size, and a whole series of tests had been made to examine that very point. Some of the other apparently greater sills did not involve additional volume, for they represented an apron shaped in a particular way, and were not to be regarded as superstructures added to an existing flat apron. That point of view was illustrated by *Fig. 18* (facing p. 29 §), which showed an early attempt to design the sill, apron, and weir as a single entity. It was interesting to hear that Dr. Rehbock had tried and discarded triangular sills in 1921, but it should not be forgotten that at that date the causes of boundary-layer breakaway were understood only by a few specialists in aerodynamics, and the subject was entirely unknown to ordinary engineers. All the more credit was due to Dr. Rehbock for his success at that date, but it followed that his finding with regard to triangular sills was correspondingly less conclusive. To-day, within certain ranges, it did seem possible to obtain the results of a dentated sill by using a simpler and more compact design on the lines of *Fig. 18*, although there were cases in which the dentated sill undoubtedly had the advantage, particularly when the apron was already in existence at too high a level.

The Authors thought that moderate scour had very little influence upon the risk of piping. The under-leakage was mainly controlled by the regions in which the streamlines were most congested, and the small reduction in the length of leakage-path with any bed-configuration that they had accepted as safe would only cause a minute increase in leakage. On the other hand, scour very close to the toe was dangerous, for not

only did it leave the structure locally unsupported, but it might also remove one of the regions of congestion, and so cause piping.

A very dangerous type of motion occurred when the submerged jet remained separated from the weir and apron by a large bottom roller. That only occurred under very critical conditions, and might easily be missed if the tests with a model were insufficiently exhaustive. The Authors had made many hundreds of experiments, and yet had not found such motion until one day it was disclosed by the small model used for the photograph, *Fig. 18* (facing p. 29§). Once aware of it, they had reproduced it fairly easily in the larger model. They thanked Dr. Rehbock for calling attention to that danger; perhaps they ought to have done so themselves, but they feared making their Paper too long. For the same reason they had restricted the description to two-dimensional models, but, with Mr. Doran and Dr. Rehbock, they would emphasize that further important difficulties were met when the complete three-dimensional model was tested.

Paper No. 5157.

"An Aerial Survey of the Estuary of the River Dee, employing a Simple Method of Rectifying Oblique Photographs." †

By JAMES LOUIS MATHESON, M.Sc., Assoc. M. Inst. C.E.

Correspondence.

Mr. Jack Allen pointed out that the value and ingenuity of the method devised by the Author to produce a map from the given photographs of the Dee estuary were especially apparent after the abortive attempts made by other methods, such as that of projecting the photographs upon an inclined screen by means of a lantern. Mr. Allen and two research students had used the Author's method to make a plan from another set of photographs of the Dee estuary, taken in September, 1938. Their experience had confirmed the conclusions set out in the Paper regarding the distance of the eyepiece from the lantern slide, the degree of accuracy obtainable, and the desirability of well-defined control points. It was important to realize that in plotting the final map as a weighted average of projections from different photographs, the accuracy might be improved by continual reference to the photographs and the use of the principle that collinear points on them were bound to be collinear also in the plan. Certainly the method would not yield the accuracy of more elaborate

§ *Ibid.*

† Journal Inst. C.E., vol. 10 (1938-39), p. 47 (November 1938).

aerial surveys, but Mr. Allen was convinced that it had a definite field of application in the rapid and comparatively cheap production of a reasonably accurate map of non-permanent features, such as the channels of an unstable estuary. The photographs of the Dee were of very great interest apart from their adaptability to cartography, in that they exhibited the essential ripple formation of the sands. That important feature was, in fact, demonstrated more graphically than if the photographs had not been so oblique. Not only was there visible a large number of sand ripples having a wave-length of approximately from 40 to 70 yards, but indeed certain of the banks appeared to be composed of great tidal ridges lying in folds across comparatively flat shoals. Some of those major ripples were clearly several feet high from trough to crest and $\frac{1}{4}$ mile or more apart.

Mr. H. F. Molony was well aware of the extreme variations that occurred in the positions of the banks in the lower part of the Dee and its estuary. He would like the Author to explain the purpose of the investigation which led to the survey described in the Paper, since it would appear that no more than approximate accuracy was aimed at or achieved, whilst the results were severely limited in application as they were demonstrative of the state of the estuary on a certain date. Owing to the conditions that prevailed in the area surveyed it would be possible that a survey carried out, either by the same or any other method, on another date might record very different positions of the channels and banks; therefore, the value of any survey of that estuary was restricted to showing the conditions which ruled when it was carried out. Indeed, as correctly pointed out by the Author, the sandbanks in the Dee estuary varied so much in size and position that it was practically impossible to survey them with any accuracy by ordinary methods; in fact, their extreme fluidity in regard to location raised the question why any attempt to survey them in more than a general way should be made. The area dealt with by the Author was, therefore, admittedly difficult to survey, and although suitable for the method employed the positions of the banks could not be checked by actual measurements. Surely a more satisfactory trial of the method employed would be over flat country with well-defined control points?

It would add to the value of the Paper if the Author explained how the camera was fixed in the aeroplane, and how the angle of inclination was measured as accurately as half a degree.

The evident care and ingenuity displayed in plotting made it regrettable that more accurate results could not be achieved. The cost of the survey was so moderate that it was a most attractive method from the point of view of outlay.

The Author, in reply, pointed out that the purpose for which the survey was made was to provide a basis for the tidal-model investigation described by Mr. Allen in his Paper "Schemes of Improvement for the Cheshire Dee: An Investigation by means of Model-Experiments*." It

* Journal Inst. C.E., vol. 12 (1938-39), p. 30 (June 1939).

would therefore be appreciated that the accuracy obtained by the method was sufficiently close for that particular purpose. The fact that the survey only recorded the state of the estuary at a certain time (low water, ordinary spring tides) was not a material disadvantage to the model-work, although admittedly it made the task of assessing the accuracy of the map more difficult. It was with that in mind that the laboratory work described in the Appendix to the Paper was undertaken, when an attempt was made to assess the accuracy obtainable under ideal conditions.

The angles of inclination given in the Appendix referred to those laboratory photographs, when the camera was supported on a tripod; the aerial photographs were taken with the camera held in the hand, and no measurements of any kind were made. The actual inclination of 5 degrees mentioned in the Paper was deduced from the angle of the tracing sheet after alignment had been completed.

Paper No. 5131.

“The Treatment of Septic-Tank Effluent from a Small Community by means of an Enclosed Biological Filter at Bloemfontein, South Africa.” †

By HUGH VAUGHAN DAVIES, Assoc. M. Inst. C.E.

Correspondence.

Mr. N. Paul Jasper could not agree with the Author that the type of plant described was ideal for villages of up to 500 inhabitants. For a small population the Author's scheme appeared to have too many working parts that required attention: (1) the pump for throwing the effluent up into the filter; (2) the fan for blowing air through the filter; and (3) the revolving distributor. That method of purification was satisfactory provided that the installation was large enough to allow a man or men to be employed daily to attend to the pumps, sprinklers, sludge- and humus-beds, and sedimentation-tanks. In small schemes it was unusual for one man to be daily employed, and, as a result, the installation was neglected and soon got out of order.

Mr. Jasper considered that the site should always be examined for the purpose of ascertaining the nature of the soil, and also the relative gradients of the surfaces, but the Author did not refer to those essential factors. If the soil were suitable for irrigation, with plenty of land available and

† *Journal Inst. C.E.*, vol. 10 (1938-39), p. 55 (November 1938).

with satisfactory gradients, treatment of the sewage by either broad or sub-soil irrigation would be advisable. If the soil were unsatisfactory—being, for example, clay—but the gradients were satisfactory, then biological filters should be installed.

The Author found that the best effluent could be obtained if the tanks were made to hold 1 day's flow of sewage. Further, he arranged his tank to contain three separate compartments, and to have a level bottom. Mr. Jasper's practice was to make provision for $\frac{3}{4}$ day's flow of domestic sewage, free from surface and rain water, with the object of not getting the effluent too septic, and hence too odorous; further, he did not use separate compartments, but advocated one long tank with baffles at intervals, with a 3-inch open space between the bottom of the baffles and the floor, with the latter laid to a gradient of about 1 in 7 (the deep end being at the inlet end). Assuming the tank to be 50 feet long by 4 feet 6 inches wide, with an average depth of 5 feet 6 inches the inlet end would be 9 feet deep and the outlet end 2 feet deep. The baffles, in addition to the open spaces of 3 inches below them, would have 1-inch horizontal slots at various levels between the floor and the water-line.

The first baffle would be 4 feet 6 inches from the inlet pipe. In that compartment the greater bulk of the solids of the sewage was collected and formed a scum, which, after it had got rid of its gases, fell to the bottom of the tank as sludge.

The bottom of the first compartment was constructed somewhat in the nature of a cone, from the bottom of which a 4-inch cast-iron pipe was carried up on the side of the tank to an outlet junction 2 feet below the water-line, from where it was carried through the wall of the tank and fitted with a valve in an inspection chamber. At intervals of 2, 3, or 4 months, as was found best by experience, the valve was opened, with the result that the water-level of the tank fell and forced up the accumulated sludge, which then travelled to the sludge-drying beds if sufficient fall were available. As soon as sludge ceased to flow, and liquid only was escaping, the valve was again shut, and the work of sludging was over for another 2, 3, or 4 months.

The second baffle was 15 feet away from the first, and the third another 15 feet away, so in all there would be three compartments of 15 feet and one of 4 feet 6 inches, and the thickness of the baffles made up the total of 50 feet. The baffles were constructed of creosoted planks fixed loosely in vertical channels and pinned down to prevent them from floating. That method of dealing with the sludge obviated the objectionable manual removal of the sludge (p. 56§). The scum and sludge which collected in No. 2 compartment was thinner and less in volume than that in compartment No. 1, and that Nos. 3 and 4 was again very much less; the sludge was removed, if necessary, by squeegees, operated from manholes

§ Page numbers so marked refer to the Paper (Footnote (†), p. 273).—SEC. INST. C.E.

placed in the tank cover mid-way between the baffles, pushing it back into No. 1 compartment. In suitable positions the tanks might be covered with 9-inch by 3-inch planks laid across the width of the tanks so that the planks could be removed for easy working of the squeegees.

The inlet-pipe to the tank was similar in construction to that shown by the Author, but it only dipped 3 inches below the water-line, and immediately beyond was fixed a board dipping 4 inches below the invert of the inlet-pipe, with the object of spreading the incoming sewage across the whole width of the tank. At the outlet end of the tank a level weir was fixed, and immediately in front of it a scum board was placed dipping 6 inches below the weir-level. The object of the weir, the scum boards, and the slots in the baffles was to allow, as far as possible, the sewage to flow across the whole width of the tank, and so to prevent any "dead" spots.

Mr. Jasper usually arranged such a tank to deliver into a chamber fitted with an automatic siphon, or siphons; if the gradients would allow, that chamber might be 8 feet square and the effluent 17 inches deep, so that the chamber would hold 542 gallons, or little over one-eighteenth of a day's flow. If the gradients would not allow of the 17-inch depth of effluent it could be reduced by as much as 5 inches. In that case the superficial area of the siphon chamber could be increased to give the required volume, and two 7-inch siphons, working simultaneously, could be fixed instead of one 10-inch siphon.

On the discharge of the tank by means of the automatic siphon, or siphons, the effluent was delivered into a channel at right angles to the tank, 35 feet long and 3 feet deep with a 6-inch half-channel at the bottom, and concrete benchings, so that the channel at the top might be 1 foot wide.

At right angles to that channel 4-inch cast-iron pipes were fixed through a funnel-shaped opening, and carried through the enclosing wall of the channel and laid horizontally across the filter. The pipes were each carried through the far end of the filter-bed enclosing wall, and were fitted with a screwed plug. Those pipes were drilled at 6-inch intervals, the holes being above the horizontal diameter and $\frac{1}{4}$ inch in diameter internally, increasing to $\frac{1}{2}$ inch diameter externally.

The filter bed itself was composed of hard clinker of varying sizes, coarse at the bottom and finer towards the top. The bed was 35 feet long by 25 feet 6 inches wide on the inside of the enclosing walls, and the clinker media had an average depth of 4 feet 9 inches, and was deposited on the usual draining tiles laid with a fall of 6 inches across the width of the bed to a number of 4-inch pipes carried through the outlet end wall of the filter, to deliver to the land for final treatment.

The channel into which the contents of the automatic tank was delivered was provided with framed wooden covers or with cast-iron grease-seal manhole covers. The distributing pipes were also covered with boards laid parallel on top of the pipes and covered with small-size clinker to a depth of 9 inches.

On the discharge of the siphon in the automatic tank, the effluent ponded up in the channel and provided a head which caused the effluent to discharge through the holes in the distributing pipes, in the form of a cascade over the whole of the surface of the bed, which, if held up, would have a depth of approximately $1\frac{1}{2}$ inch. The effluent falling through the media drove the air enclosed in the bed before it, and out through the 4-inch pipes, and atmospheric pressure forced air through the fine clinker and open joints of the boards of which the bed was covered. In that manner the bed was supplied with air to carry on its work of purification of the effluent. No objectionable smell was observed from the filter, as even when the siphon was discharging the channel covers and boards covering the filter and clinker over them prevented the escape of objectionable odours.

The effluent passing through the 4-inch outlet pipes of the filter was made to deliver, if the gradients allowed, into a system of sub-irrigation drains surrounded with rubble or clinker and covered with soil to a depth of from 2 to 3 feet, and in that manner the effluent received its final treatment. If sufficient area of land were not available for that final treatment, the effluent had to discharge direct into a ditch, stream, or river. In such a case, if the gradients allowed, two beds should be provided; if, for example, 10,000 gallons of sewage were to be dealt with, two filters each having half the capacity of the filter described should then be installed, and the effluent from the first filter should deliver into a second automatic siphon-chamber, complete with channel and distributing pipes, so that the effluent might pass through two bacterial filters before discharge.

It would be noticed that there were no moving parts, and, instead of having only a few sprinkling holes in the revolving distributor described by the Author, there were approximately 1,600 sprinkling holes, and those were so formed that with the minimum of attention they never became choked with colloidal matter. The enclosed filters were free from objectionable odours and flies, and were not prevented from working during periods of severe frosts, as were small open revolving distributors.

The percolating filters could be free from enclosing walls if the gradient of the ground were sufficient, but in such cases channels would have to be provided for holding up the effluent from the siphon-chambers, in order to provide a head on the effluent, and to allow the pipes to discharge as a cascade over the area of the filters; further, piers would have to be provided for holding up the sprinkling pipes so as to keep them level.

The "Prüss" filter described by the Author contained 50 cubic yards of media for the treatment of 10,000 gallons of effluent per day; that was to say, 200 gallons of effluent per cubic yard per day, whereas the filter Mr. Jasper had described could treat 60 gallons of effluent per cubic yard per day, the effluent being from domestic sewage free from surface water or rain.

The Author made no reference to the disposal of rain- or stormwater, and Mr. Jasper concluded that he provided only for the purification of

the water-borne domestic sewerage of the military encampment of 500 men on the outskirts of Bloemfontein. In the type of plant described by Mr. Jasper, stormwater tanks and sewers would be provided to comply with the requirements of the Ministry of Health, and to prevent scouring of the sedimentation tank, which would have passed the accumulated sludge in the tank on to the filter bed and choked it.

*** Owing to the outbreak of hostilities, the Author's reply has not been received in time for insertion here. It is hoped to publish it later.
—SEC. INST. C.E.

Paper No. 5156.

“Model-Experiments on Bellmouth and Siphon-Bellmouth Overflow Spillways.”†

By GEOFFREY MORSE BINNIE, M.A., Assoc. M. Inst. C.E.

Correspondence.

Mr. W. J. E. Binnie, President, observed that, owing to the rare occurrence of floods of any great magnitude, it was seldom that an opportunity arose of comparing actual results with those predicted by model-experiments, and therefore the following information might be of interest.

A bellmouth overflow discharging into a tunnel was constructed to dispose of flood water at the Burnhope reservoir, the overflow being designed to deal with a maximum quantity of 2,670 cusecs, whilst the effective length of the crest was 154 feet. When the reservoir overflowed the water passed over a weir 200 feet in length and entered the channel leading to the bellmouth. The water flowing from the tunnel entered a basin, finally discharging into the river over another weir 110 feet in length. Self-recording instruments were established so that H was measured for both weirs and the bellmouth.

Model-experiments were carried out to determine the coefficient C in the formula $Q = CLH^{\frac{3}{2}}$, where L denoted the length of the crest and H the depth of overflow. A flood occurred on 24th and 25th October, 1936, when the maximum value of H recorded at the bellmouth reached 1.7 foot. The crest of the 200-foot weir was similar to one which had been tested in America * and the crest of the 110-foot weir was similar to one which had been tested in Egypt ‡. The coefficients determined by those experiments were therefore adopted, and the mean value of the

† Journal Inst. C.E., vol. 10 (1938-39), p. 65 (November 1938).

* “Water Supply and Irrigation,” U.S. Geological Survey, Paper No. 200, p. 109.

‡ Report on Submerged Weirs and Standing Wave Weirs, Diagram VII, Type No. 9. Government Press, Cairo, 1923.

results obtained for the readings of H on both weirs was assumed to be correct. The value of C for the bellmouth weir was calculated for increments of 3 inches between depths of 0.25 and 1.7 foot on the crest, and was found to vary from 4 to 3.95, with the exception of one reading.

Mr. Binnie further pointed out that, owing to the turbulence which occurred in the basin into which the tunnel discharged, it was possible that coefficients derived from observations of H on the 200-foot weir might be more accurate, in which case the value of C would vary from 3.99 at a depth of 3 inches to 4.16 at 1.7 foot.

The tests carried out with the model to a scale-ratio of 1/24 indicated a value of 3.84, confirming the impression which had been formed from experiments carried out elsewhere that the prototype would give rather better results than those indicated by model-tests.

Some apprehension had been expressed that the amount of air carried down the shaft with the water overflowing the bellmouth would be so considerable that it would occupy a proportion of the cross-sectional area of the tunnel such as to reduce seriously its effective carrying capacity. It was decided, therefore, to carry out experiments at Burnhope, and an exceedingly ingenious method was devised by the Resident Engineer, Mr. S. Alderidge, by means of which the air could be measured with accuracy*.

The results of those experiments showed that the proportion of air in relation to water decreased as the volume of water increased until a point was reached when the volume of air was one-quarter that of the water, and that proportion of air had no measurable effect on the discharging capacity of the model-tunnel.

The Author, in reply, observed that, as stated by Mr. W. J. E. Binnie, the fear was sometimes expressed that the amount of air carried down a tunnel when a bellmouth overflowed might be so great as seriously to reduce the effective discharging capacity.

It would seem probable that, like surging and vortex-formation, the amount of air drawn in at the higher flows was also a function of the vacuum formed inside the tunnel. Thus if, for example, instead of constructing a vertical shaft and horizontal tunnel the same diameter throughout, the vertical shaft were constructed with a larger diameter than the rest of the tunnel, the vacuum formed inside the former would be considerably reduced, and, in consequence, the amount of air drawn in would also probably be reduced.

The Burnhope experiments indicated that under the conditions generally adopted hitherto with a vertical shaft and horizontal tunnel the same diameter throughout, the proportion of air drawn in was not excessive as compared with the discharge at the higher flows, and the results were most reassuring in regard to that question.

* Trans. Inst. W.E., vol. xlii (1937), pp. 113 and 115.

CORRESPONDENCE
ON PAPERS PUBLISHED IN
DECEMBER 1938 JOURNAL.

Paper No. 5167.

“The Principles of River-Training for Railway Bridges, and their Application to the Case of the Hardinge Bridge over the Lower Ganges at Sara.” †

By SIR ROBERT RICHARD GALES, F.C.H., M. Inst. C.E.

Correspondence.

Mr. C. C. Inglis, of Poona, proposed to divide his observations into (1) arguments leading up to the Author's conclusion that the length of approach-bank protected by a guide-bank was proportional to the length of the guide-bank; (2) slips and failures of pitched banks; (3) the design of training works; and (4) points arising out of the Author's proposals.

In regard to (1), the Author stated that:

“... it is the principal function of the heads of guide-banks to induce cut-offs, and by this means to bring back between the guide-banks the river which would otherwise have breached the approach-line . . .”

“It would seem that every alluvial river has its own particular ratio of length of bend to length of chord at which it may be expected to cut-off, and that the ratio would vary according to the characteristics of the river at the site of the bridge.”

“From the example of the Curzon bridge may be deduced the relation between the length of the guide-bank and the length of the approach-bank which can be protected by it. In Figs. 2 (a), Plate 1, the width of the waterway denoted by L is taken as the length of the bridge less half the two end spans, which are usually obstructed by the guide-banks.”

Figs. 2 (a), (b), and (c), Plate 1 (facing p. 224 §), showed cases in which the *khadir* banks were $1.65 L$, $3.5 L$, and $6.5 L$ from midstream; the semicircular embayments shown by the Author were called by him “natural radii of unrestricted curve in this part of the river,” the radii

† Journal Inst. C.E., vol. 10 (1938-39), p. 136 (December 1938).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

being 1,500 feet, 3,300 feet, and approximately 8,000 feet. From that, it was clear that the Author visualized cut-offs as depending only on the bend-chord ratio, and that whether the chord be 1,500 feet or 15,000 feet, a cut-off would occur provided that the bend-chord ratio reached the same figure. Arguing from that, he held that the maximum embayment depended on the width of the *khadir*, and that where there was no lateral restraint, such as that caused by a *khadir*-limiting bank or beds of *kunkur* or indurated clay, there was almost no limit to the "radius of unrestricted curve" or to the possible embayment. Actually, there were two other very important factors which controlled the extent of embayment: (a) the natural length of the river between two fixed points; and (b) natural meander length and amplitude, which varied as \sqrt{Q} approximately, where Q denoted the dominant discharge of the river. In the case of the Hardinge bridge the length of the Ganges between Sardah and the bridge had altered little during the period for which reliable data were available, and there were strong reasons to believe that so long as the river was held at Sardah, at Raita, and at the bridge, no marked alteration would occur.

Mr. Inglis therefore considered that the Author had made assumptions which were not justified; had not taken into account dominant factors; and had assumed an embayment between the nose of the guide-bank and the approach-bank, although the Curzon bridge data, on which his thesis was based (*Fig. 1*, p. 142 §), did not bear out his argument, in that there was no such embayment there, and that the cut-off took place without any lateral restraint. It had also to be mentioned that models at Poona (both for the right and left guide-banks) corroborated that, as shown in *Figs. 21* (pp. 282, 283, *post*). *Figs. 21* showed that, with correctly-designed Curzon-type heads to the guide-banks, embayment extended little behind the end of the guide-banks and did not curl around them towards the approach bank.

Mr. Inglis disagreed with the Author that the Gandak bridge at Bagaha was foredoomed. It was worth noting that, according to a plan supplied to Mr. Inglis, in spite of the extremely unfavourable conditions resulting from the spur-type head of the right guide-bank and to "the bridge not crossing the fall of the country at right angles," the embayment did not extend nearer the approach-bank than the nose of the guide-bank until after the groyne was built upstream of Chhitauni Ghat station in 1924. That groyne made the destruction of the guide-bank inevitable, due to its pulling the stream around its tail and creating violent turbulence; just as the construction of the Damukdia guide-bank above the Hardinge bridge made the destruction of the right guide-bank inevitable.

The Author produced evidence which, in his opinion, showed that the breach which occurred in the right guide-bank of the Hardinge bridge on the 26th September, 1933 was not due to normal causes, namely, slipping of the toe of the pitching.

He stated that, 9 months before the breach occurred,

“ . . . at chainage 20·5, afterwards to become the centre of the breach, the section from the foot of the permanent slope gives a fall of 1 in 7 for 90 feet to water's edge, followed by a steeper slope of 1 in 1·7 for 170 feet. It will be observed that the outer slope . . . is steeper than the normal 1 in 2, but that there is a large margin of from 90 to 85 feet width, with the very flat mean slope of 1 in 6·5 or 1 in 7 to go down before any slip could reach the foot of the permanent slope.”

Experience at the Hardinge bridge and with models at Poona had shown that a slope of 1 in 1·7 was not far removed from the critical slope, and that should a slip occur, it was generally found to stabilize at 1 in 2·5, or even flatter. Had a slip stabilized at 1 in 2·5, 74 feet of the 90 feet available would have gone down, and the whole of the 90 feet if the slope had stabilized at 1 in 2·7. It seemed to Mr. Inglis, therefore, that the Author was not justified in saying that there was plenty of margin for the apron to follow the scour down by small steps. Moreover, the Author continued :

“ However, no breach occurred in June or July, or in August . . . , and the river was falling steadily by the middle of September . . . , when the river ceased to fall and the rapid rise of the freshet began. . . . As the breach had not occurred by some catastrophic slip . . . when river-level passed R.L. 240·2 on the fall, there would be no reason why a slip should take place at this level on the rise.”

Actually the breach occurred at the beginning of the rise, after the water had risen only 1 foot, and the toe conditions which led up to the breach were bound, assuming step-by-step launching, to have occurred just before the rise took place; the damage, which model-experiments showed invariably occurred just below water-surface level, would then extend rapidly, exactly as occurred at the Hardinge bridge. None of the evidence produced, in fact, indicated anything abnormal in the mechanism of failure, or justified the Author in saying that it was necessary to seek some other cause for the breach.

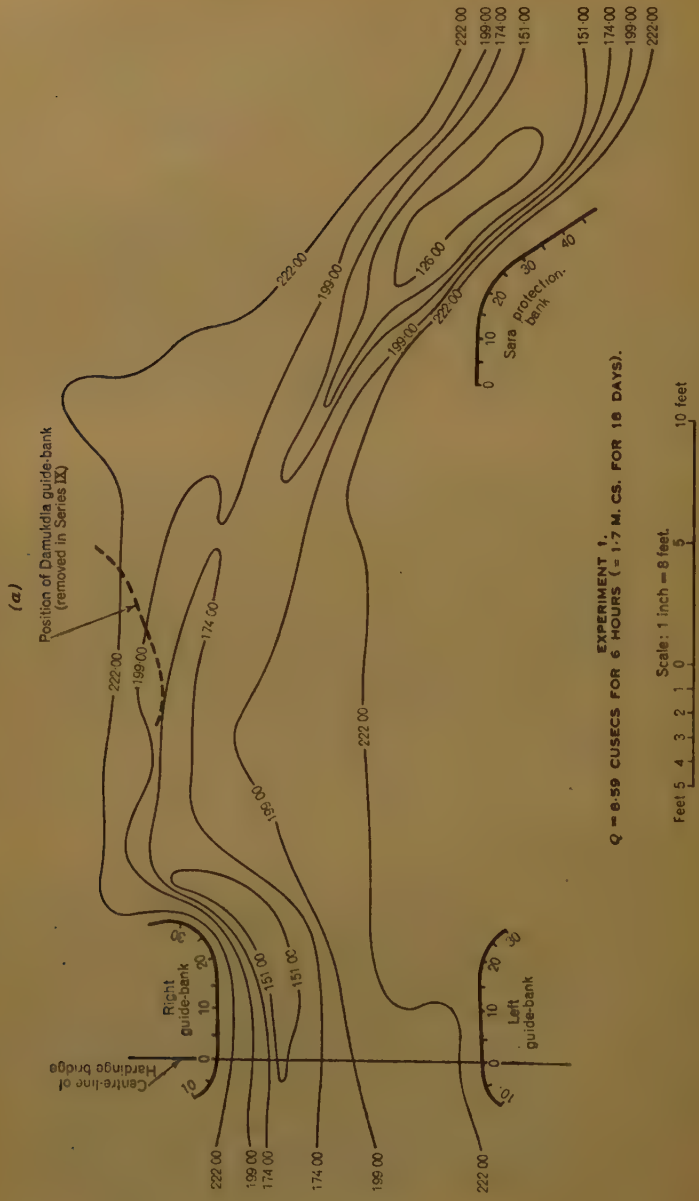
On p. 151 § the Author stated :

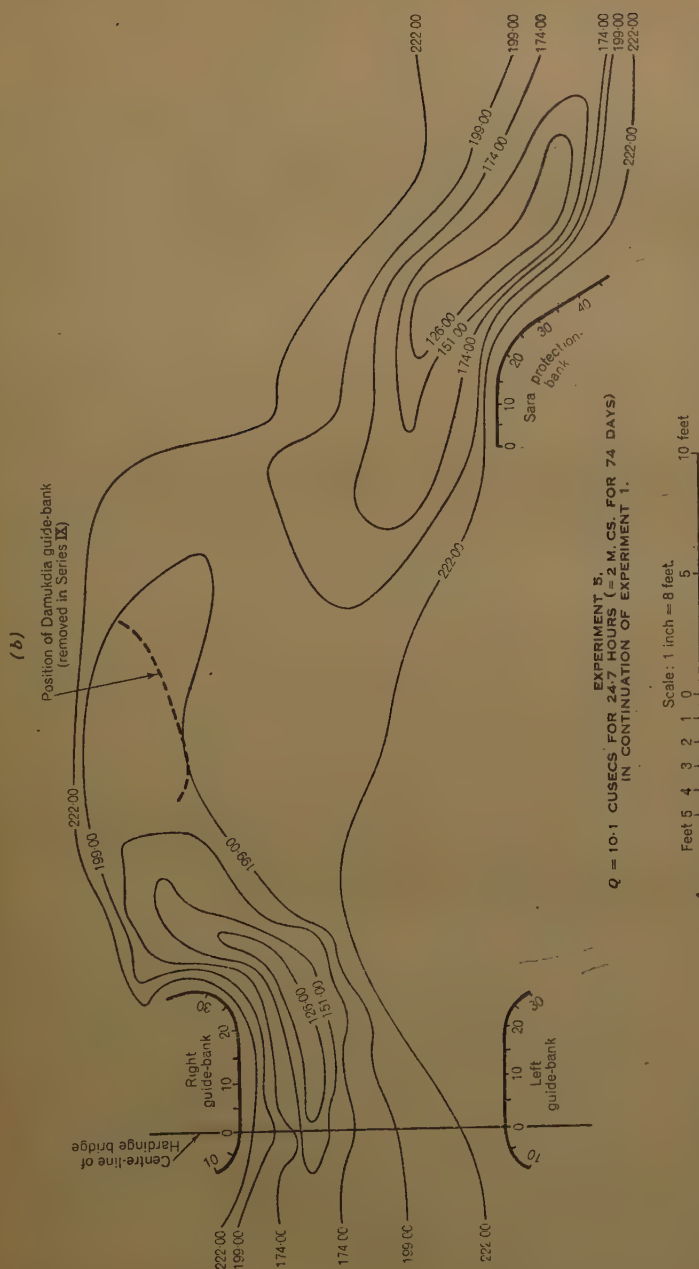
“ On the 26th September, 1933, a breach occurred in the right guide-bank, for which no convincing explanation had been put forward until it was suggested by the late Mr. B. L. Harvey, M. Inst. C.E., that the breach probably started high up on the slope of the bank, at or just below water-level, and that it was caused by the suction, created by fast flow and especially by fast-moving eddies [at the surface], drawing out silt or sand from between the pitching stones of the slope covering. . . . The attacking force was a freshet of unusual violence, which interrupted a steady fall by a rapid rise of between 4 and 5 feet in river-level *. The rise was accompanied in this part of the river by waves which are described by the observer as rushing through the gap in the guide-bank into the embayment at intervals of about 2 minutes and creating a sudden afflux of about 2 feet. It has been suggested by the present Author [Sir Robert Gales] that the failure was

§ *Ibid.*

* As already explained, the breach actually occurred at the beginning of the rise.

Figs. 21.





EXPERIMENTS WITH $\frac{1}{300}$ SCALE MODEL OF THE RIVER GANGES AT THE HARDINGE BRIDGE: SERIES IX, WITH DAMUKDIA GUIDE-BANK AND JACKSON SPUR REMOVED.

due to the sand being dragged down and drawn out by these surge-waves passing along the face of the guide-bank, after the fashion . . . of the wave caused by a large ship passing through a relatively small channel."

In regard to there being no convincing explanation of the breach, not merely had it been shown by the 550-foot-long model of the Ganges from Sardah to the off-take of the Gorai river that heavy toe-scour would occur, which would cause slips and, eventually, the breach, but it had also been shown still more clearly in a large, geometrically-similar, model of the right guide-bank, in which the breach was reproduced in detail. A demonstration model was on view for 2 years, which showed exactly how the breach occurred, and satisfied observers that the river conditions in 1933 were such that the breach was inevitable, as a result of toe-scour. The Author's comparison of the surge-waves in the river to those created by a large ship in a relatively small channel was not a happy choice, because, whereas the surge-waves, which were reproduced in the model, were due to "river-breathing," the wave caused by a large ship passing through a relatively small channel was due to pressure energy changing into kinetic energy.

On p. 144 § the Author stated, in connexion with the Elgin bridge, that " . . . an eddy set up at C_2 [Fig. 2 (b), Plate 1] has been observed on a falling river to have the effect of deepening the channel along the [right] guide-bank," and on p. 189 §, he mentioned that the main stream of the river was driven across to the right bank by the whirlpool at Sara.

Model-experiments showed that such eddies, which generally were found near places where slips occurred, were merely superficial, extending down to about half depth only, and that they resulted from surface water, flowing away from a guide-bank at an angle, "brushing" past the relatively stationary surface water near the guide-bank, causing it to revolve with a low velocity. So far from such eddies and surge-waves being the cause of damage, they were both secondary effects, the latter being due to what Mr. Inglis described as severe action at the bed, at the toe of the pitching, where the flow was straight and turbulent, coming in surges, some stronger than others. Stones were carried away in the surges, and although a stone might remain unmoved for several seconds, it might then be carried away in a powerful surge*. Those surges were due to high-velocity bed-water, with a very steep velocity-gradient, creating unstable conditions like those in the wake of a badly-designed steamer.

There was therefore no convincing evidence, either direct or indirect, to lead to the belief that the breach had been caused by extraordinary conditions. Undoubtedly, the breach originated just below the surface, as breaches always did, but that was due to all the available apron stone

§ *Ibid.*

* *Correspondence on the late Mr. B. L. Harvey's Paper, "The Restoration of the Breach in the Right Guide Bank of the Hardinge Bridge." Journal Inst. C.E., vol. 6 (1936-37), p. 330 (October 1937).*

having launched, so that when a further slip occurred at the toe, stone from above rolled down the slope to make good the shortage, leaving a bare patch just below the surface, which extended rapidly. That was what had occurred at Poona in every experiment with a launching apron, and in no case had there been any other explanation; bearing in mind that the models showed that the breach was inevitable, there was no justification for assuming something extraordinary, which had no physical explanation. A Technical Paper dealing with falling aprons was in course of preparation at Poona.

Mr. Inglis would next consider what the Author called the "catastrophic slip" of the 25th October, 1934, in which "a flat tract of apron, 500 feet long and varying from 100 feet wide at chainage 5.5 . . . to nothing at chainage 10.5, went down bodily within a few minutes, the slip extending deeply into the bank." He agreed with the Author that ". . . this slip . . . was caused by the deepening of the channel along the toe of the apron," and that ". . . the reason why two-thirds of the width of the apron went down in one slip instead of in a succession of small slips" was that clay-patch strata were met with. In spite of that experience, however, the Author still seemed to consider that the Sara clay bank should be held.

When the local engineers found from experience that holding a clay bank against a severe attack was exceedingly difficult and expensive, they asked the Poona Station to carry out experiments to ascertain how launching occurred and why results proved to be so unsatisfactory. The model was a geometrically-similar representation of the Sara bank conditions, the strata consisting of layers and pockets of clay and sand*. The experiments showed that instead of the apron launching equally, as it did when laid on sand, the clay eroded into a series of steep cliffs and flat beds, so that stones which fell over a cliff landed on a flat bed of clay and were then mostly washed downstream; that was partly because stones were easily swept along a smooth surface, but also because turbulence was great, owing to the unevenness of the clay faces. At best, therefore, large quantities of stone would be required to stabilize the slope, and where turbulence was great it was well-nigh impossible. From that it followed that clay bluffs should not be used as a foundation backing for aprons, although they might be useful as a second line of defence, with the protection-bank well in front, provided that there was sand down to the eventual toe of the pitching.

In regard to the design of training works, the Author stated on p. 188 § that :

"in view of the fair alignment and reputed stability of the left bank", siting the bridge below Sara "appeared to offer immunity from any embayment of the

* Annual Report for the Year 1937-38 of the Central Irrigation and Hydrodynamic Research Station, Poona.

§ *Ibid.*

river at the back of a flanking guide-bank on either the right or the left bank of the river. In other words, the river appeared to be held in complete control by the Sara clay, subject only to periodical sequence of bend and cut-off. . . . In these favourable circumstances it was considered that there would be no necessity for the upstream length of the flanking guide-banks to exceed three-fifths of the waterway, and no need for the heads to be curved back more than 60 degrees."

Mr. Inglis found it difficult to reconcile that with the statement on p. 186 § that the head of the Raita peninsula, by means of cut-offs, had been eroded a distance of about 2 miles, and the Sara clay had similarly been evenly eroded to the extent, variously estimated, of perhaps $\frac{1}{2}$ mile. Even if the figure of $\frac{1}{2}$ mile of erosion in 130 years (from 1780 to 1909) were accepted, it was clear that when those banks were attacked, erosion and embayment would begin; and once started, they would increase rapidly upstream, because a guide-bank tended to pull a river around its nose †. That was exactly what happened; the Author stated that after the bridge had been opened for traffic the bank at Sara, which had been occupied since the beginning of the railway by the left-bank ferry station, had been revetted with pitching stone for a length of 650 feet. As soon as that was done, the bank immediately upstream of the revetment began to be eroded. Had the Author not realized the importance of the curving back of the heads of protection-banks, the omission of that safeguard could be understood; but on p. 137 § he stated that:

"The Author's principal contribution to this system [the bund-and-apron method] of river-training was the introduction of the curving back of the head of the Bell bund to such a degree as to ensure that any bend of the river, at its deepest embayment . . ., would encounter a continuous stone-pitched bank offering no interference with smooth flow."

On p. 189 § the Author stated that:

" . . . by 1932 the river had eroded the clay bank above the end of the protection-bank to a depth of embayment of $\frac{1}{4}$ mile and was running across the partially submerged wreckage of the end of the protection-bank in a direction which threatened a deep embayment above the head of the right guide-bank."

Even then, however, the situation could have been saved had the head of the right guide-bank been curved back to form a Curzon-type head, as demonstrated by model-experiments*, and shown in *Figs. 21 (a) and (b)*, (pp. 282, 283, *ante*), which showed that the loop of the embayment would not have extended below the end of the guide-bank. That result was to be expected from the fact that where the attack came from the opposite bank a relatively short guide-bank would suffice, just as a longer guide-bank was required, as pointed out by the Author, where the attack swung in from the same bank. Instead of adopting that cheap and simple solution,

§ *Ibid.*

† Footnote (*), p. 284.

* Paragraphs 2 and 3 of "Final Note for the Hardinge Bridge Committee", p. 39. Bombay P.W.D. Technical Paper No. 55: Hardinge Bridge Model Experiments.

however, the Damukdia guide-bank was constructed (Fig. 14, Plate 2, facing p. 224 §), "which ignored the first principle of river-control—namely, that opposing the direct force of flow caused relatively harmless kinetic energy to be changed into highly destructive turbulent eddy-flow of an unstable kind, accompanied by surging *," which rendered the destruction of the right guide-bank inevitable.

Although the Hardinge Bridge Committee decided to remove the Damukdia guide-bank, they were determined that it should be retained and held until the right guide-bank had been extended. That decision having been taken, it was essential to minimize the attack on the right guide-bank, which was the main problem referred to the Poona Station for solution. Several alternative methods of getting the water to flow past the Sara bank in such a way that flow downstream would be relatively smooth and natural were tried †, but eventually, mainly to save expense, as much stone as could be removed from Sara bank was recovered, and the river was left to erode the protruding portion, which was the direct cause of the attack concentrated towards the Damukdia protection-bank and right guide-bank.

There was no doubt that the action taken fulfilled its object, but the Author held that serious results had followed, and that the position had been lost from the point of view of limitation of bend-erosion by cut-off. To quote from p. 196 § :

"The left bank between Bangalpara and Dhapari was completely submerged during the high flood.' . . . The submergence . . . appears to have been brought about partly by the direction of flow and concentration of the river at Raita, and partly by the destruction of the upstream portion of the Sara protection-bank. Confirmation of this conclusion is supplied by the change in the ratio of bend to chord from 1.9, when indications first became definite, to 1.6, at the present time. Nevertheless, to show by what a narrow margin safety was missed, the Report may be quoted as follows : ' . . . if the main stream at the Raita protection bank straightens a little more and presses against the protection bank about 400 feet further downstream before the flood subsides, in all probability the Damukdia channel will open this year [1938].' The serious results which have followed from a lateral erosion of 800 feet at the curved part of the dismantled Sara protection-bank, and the fact that any practical restoration would entail further retirement, are sufficient to show that the position has been lost."

In reply, it might be stated that, if the erosion of 800 feet of the Sara projection had had any effect on water-levels, it had reduced the afflux upstream, and hence it had, if anything, decreased flooding. The only way, in fact, in which the erosion at Sara could have increased upstream levels would have been by delaying the occurrence of the cut-off; that, apparently, was what the Author meant. The answer to that was clear: no cut-off could have occurred until the river above Raita had translated sufficiently far downstream to clear the Raita protection-bank, because until then, curvature conditions below Raita were highly unfavourable to

* Footnote (*), p. 284.

† Footnote (*), p. 286.

§ *Ibid.*

a cut-off; but as soon as the translation reached the tail of the protection-bank, conditions would become favourable to a cut-off. In other words, the cut-off below Raita was determined by translation above Raita, which the experiments showed had been little affected by changes at Sara. Had the Sara protection-bank been designed correctly in the first instance, it would probably have given little trouble. By the time the upper end had become a submerged spur, however, and upstream scour had caused the unscoured portion of the bank to protrude into the river, the position had been lost, because, as already explained, where the substrata consisted of layers and pockets of clay and sand, it would be very expensive, and well-nigh impossible, to hold a protection-bank against severe scour although, where scour had occurred to a great depth and silting had subsequently occurred, it might be practicable to build a new bank in front of the old position.

In regard to (4) (points arising out of the Author's proposals), Mr. Inglis would mention (a) the question of a suitable waterway for railway bridges; (b) the optimum size of pitching stone; (c) the thickness of the stone face on a slope after the launching of the aprons; and (d) the splay of guide-banks.

With regard to (a), Mr. Gerald Lacey had shown that the wetted perimeter of regime channels was equal to $2.67\sqrt{Q}$, and that for practical purposes the waterway of a bridge might be taken as equal to the wetted perimeter for maximum discharge*.

Referring to (b), model-experiments showed, as would be expected from theory, that the larger the stones used the less the danger of their being washed downstream, and that a mixture of sizes gave better results than a single uniform size.

With reference to (c), the Author's idea that the thickness of the layer of stone remaining on a slope after an apron launched could be regulated by the distribution of stone in the apron had been shown by experiments to be incorrect. The thickness of stone was almost uniform, and depended only on the size of stones and material underlying the launched apron. If a thicker apron were considered desirable, extra stone would have to be dropped from barges; but if, subsequently, further launching took place the launched stone would thin out again to the natural uniform thickness. Large blocks with flat faces were objectionable, as they created severe action.

In regard to (d), downstream of a gorge a river fanned out naturally. At the gorge the river was much narrower and deeper than in a regime section, and therefore expanded owing to its natural tendency to attain a width at least equal to $2.67\sqrt{Q}$. It then continued to expand, usually splitting into two channels. Generally, therefore, a position could be

* "Stable Channels in Alluvium." Minutes of Proceedings Inst. C.E., vol. 229 (1929-30, Part 1), p. 259.

found where the river was naturally equal to, or a little wider than, the regime width, where a bridge could be built with no fear of subsequent deep scour. Examples of that were the proposed railway bridge at Amingaon across the Brahmaputra †, and the Sukkur barrage ‡. Although such conditions were found in nature, they would be well-nigh impossible to reproduce artificially, because the discharge-intensity (discharge per foot of width) was unnatural for the total discharge of the river, and under such conditions the sides were nearly vertical—as compared with 1 in $2\frac{1}{2}$ or flatter, required where the banks were pitched—so that they would not stand. In practice, therefore, the entry was bound to approximate to regime width; under such conditions there would be little tendency for the river to expand, and flow would concentrate along one bank or the other, whether the guide-banks tapered or not.

There was little difference in the basic principle, whether it had to be applied to a railway bridge or a canal headwork, and Mr. Inglis believed that he was correct in saying that both railway and canal engineers normally designed their guide-banks as parallel.

Guide-banks which converged downstream would be less effective in bringing about cut-offs, but it should be clearly understood that a cut-off resulted not so much from the change in conditions at one point, as the integrated effect of changes occurring upstream; and even though a bend-chord ratio which appeared to be highly favourable might be created at a point, no cut-off would occur until the river upstream had changed sufficiently to render the cut-off possible; conversely, though the bend-chord ratio at a point might not appear to be highly suitable for the occurrence of a cut-off, nevertheless it would occur if conditions upstream changed so as to favour the change-over.

In regard to the Author's statement that "splayed guide-banks were anathema to the railway engineer for reasons too numerous to be detailed here", Mr. Inglis asked him to give a full list of those reasons.

The Author stated (p. 202 §) that :

"It would be observed that the collection of the river by means of two widely splayed guide-banks, although quite unsuitable for a railway bridge, had become a fixed idea on which the model-experiments were founded. It subsequently had appeared that with the commencement of the model-experiments, the initiative in the task of remodelling the training works had passed to the Poona laboratory, where the virtues of converging guide-banks were evidently quite unknown."

Actually the Committee did not recommend the collection of the river by splayed guide-banks; on the contrary, they proposed parallel guide-banks of length equal to the waterway, whereas the Author proposed a design in which the guide-banks were slightly splayed. It was also entirely incorrect to say that "the initiative . . . had passed to the Poona laboratory." Mr. Inglis was neither on the Committee nor was he

† Specific Note No. 38 of 1939. Poona Research Station.

‡ Footnote (*), p. 285.

§ *Ibid.*

called in as a consulting engineer. His work, except in regard to detail, was laid down by the Committee, and prior to the Report, the only experiment carried out by him independently of the Committee was Series IX, in which a Curzon-type head was added to the right guide-bank and the Damukdia bank removed, to show that under such conditions the river would embay naturally without endangering the approach-bank or bridge in any way. Mr. Inglis had no part in the Committee's recommendation to prepare estimates for retiring the main line in case that should be found necessary at some future time. Actually, the Committee thought it unlikely that such a retirement would be necessary, and in any case it would be a cheap solution.

Although he disagreed with many of the Author's opinions, especially in regard to details and the application of basic principles to specific cases, Mr. Inglis had no objection, on hydraulic grounds, to the elongated guide-banks recommended by the Author; it would, however, be very difficult to hold the Sara bank unless an advanced alignment was practicable, and even if it were, the design as a whole would be very expensive.

In conclusion, Mr. Inglis wished to express his admiration for the work of the great river engineers of India, including the Author. Those men, with little to guide them except surface indications, had, as a result of intuition, been able to unravel many of the secrets of river-training; but the great advances in hydraulic science during the past 25 years, combined with a powerful new weapon of attack—model-experiments—had shown that some of the earlier ideas of what occurred below the water surface were by no means correct. Composite models, consisting of rigid and non-rigid portions, did not give an exact reproduction of what would happen in the river, because different laws held for the rigid and non-rigid portions. If they were properly planned, however, with correct entry-conditions, the model-results represented the equivalent of what would occur in the river, and could be correctly interpreted. Mr. Inglis greatly regretted that he had not had an opportunity to show the models to the Author; he could, however, assure him that others, who at first had had just as little faith in models as the Author, had been satisfied as to their value when they had seen them under test.

Mr. H. A. McGillicuddy, of Buenos Aires, observed that there was a striking similarity between the rivers in the north of the Argentine Republic and those described in the Paper. In both cases the rivers were building up their beds, and their waters were laden with more solid matter than the current was able to transport. From those two facts arose most of the phenomena which affected the problem of training.

Apparently in India as well as in Argentina, the early attempts at river-training were based on the use of solid spurs built either of stone or of sand faced with stone, and in both countries the results were unsatisfactory. In India the problem had been solved by the introduction of the Bell bund or guide-bank. In Argentina a system of permeable training

works had been developed which had proved highly satisfactory, both in performance and cost.

Bell bunds or guide-banks had not been used in Argentina; the larger rivers had not yet been bridged, and for the smaller ones either the physical conditions did not exist, or the saving in the length of the bridge which might be attained did not appear to justify the high cost of the Bell system.

The theory underlying the system of permeable training works was briefly:

In time of flood the water was loaded to saturation point with solid matter in suspension, the amount of solid carried depending on the velocity of the current: any slowing down of the current, therefore, would produce immediately a deposition of silt. The training works had to be so designed that they offered very little resistance to the force of the current, in order that they might neither themselves be overthrown nor tend to cause the formation of eddies, but they had to produce just that necessary amount of slowing down which would cause the silt to deposit.

It was characteristic of rivers crossing alluvial plains that the quantity of solid matter in suspension was always in excess of what the current could transport, and rivers never refused the invitation to decant silt in a place prepared for the purpose by the construction of permeable training works.

The type of training work which had been found most suitable consisted of a group of spurs or fences placed normally to the current, and made by driving old rails into the sand, leaving 5 or 6 feet protruding. The rails were spaced 5 or 6 feet apart and between them were strung three wires, any old wire or cable available being used. A layer of brushwood was attached to the wires. Those spurs would sometimes produce in one season a filling greater than their own depth, but it was generally found wiser not to attempt to carry out all the required filling in 1 year, as in the second year the spurs could be lengthened and raised and the filling thus made to continue. The river had to be given time to adjust itself to the new conditions and to scour out a new channel to replace that which was being filled in.

In reading the Paper and Discussion two points had impressed him. The first was that apparently no system of outer training works were used in India to lead the current to a fair entrance between the guide-banks; in fact, an extraordinary amount of deviation from the general direction of the current, and deep embayments of the channel at the entrance to the guide-banks, were tolerated. Curved heads, turned back at more than 90 degrees, enabled the channel formed by the guide-banks to receive the current entering at a very oblique angle, but those curved heads were bound to increase the cost of the guide-banks, whilst the obliquity of the current would tend to induce scour, the formation of

eddies, and slack water, in which sand-banks might form. Incidentally, outer training works and the maintenance of a straighter channel would relieve the guide-banks of the duty of protecting the approach banks, for which purpose apparently they were sometimes made longer than would otherwise be necessary. The second point was that apparently the use of permeable training works had not found favour in India.

He would not attempt to make any comparison of merit between the two systems. Their functions were quite different, and although in many cases the same results might be obtained, the methods of working were distinct. Put very briefly it might be said that Bell bunds, as a type of solid training works, induced scour, whilst permeable training works induced deposition. With solid training works it was possible to predetermine the exact location of the scour, and, in fact, to form a channel of definite dimensions with geometrical margins and a smooth surface, ideal for passing the maximum volume of water with the minimum of eddy, through a space which was restricted by the abutments of a bridge. With permeable training works it was possible to induce filling and to predetermine, with a fair amount of accuracy, the location of that filling. As a result of the filling scour would take place, but the location of that scour could only be predetermined approximately. With permeable training works it was possible to change the position of any channel or to hold a channel in any desired line, to prevent erosion of banks, and to close any undesired channel or spill; in fact, it was possible to constrain the river to follow a course which, although its margins might not be geometrically defined, could be predetermined and maintained with a surprising degree of accuracy. The first cost of permeable training works was but a fraction of that of solid constructions, and there was practically no maintenance cost.

It would be rash to say that permeable training works would replace guide-banks, but it was quite certain that the two systems could be used in combination, and that the addition of cheaper permeable works would make possible an appreciable reduction in the solid constructions, and consequently in the cost of the whole scheme. Further, as the cheaper works could be carried much farther afield, making possible the maintenance of a straight channel above the guide-banks, that would be a guarantee of stability for the whole system of protection-works, and would decrease greatly the cost of maintenance. It was of interest to consider how permeable training works might be used in conjunction with existing solid constructions.

In the case of the Curzon bridge (*Fig. 1*, p. 142 §), the deep embayment above the head of the guide-bank could be prevented, the cut-off shown, which on account of its steeper gradient was almost certain in time to become the main channel and to require costly protection works on the

south bank of the river, could be closed, and the present channel on the north bank could be kept straight from point 9 to the bridge.

In the case of the Gandak river at Bagaha (*Fig. 3*, p. 145 §), suitably designed permeable training works would have prevented the deviation of the current to the west bank of the river, with the resulting short-circuiting of the bridge; further, even to-day, in spite of the extent of the damage and the formidable character of the river, Mr. McGillycuddy thought that it would be perfectly feasible to restore the current to the east bank and to constrain it to pass through the bridge. He had dealt with an exactly similar case, although on a much smaller scale, with complete success. Incidentally, the strength of the current was not so much to be feared with permeable training works, as such works did not increase scour, whilst the stronger the current the heavier was the silt transported, and therefore the more solid the bank which would be formed.

The embayment of the river on the west bank at the Kosi bridge (*Fig. 5*, p. 149 §), which had driven the railway into a precarious location on a narrow strip of land between the Kosi and a spill of the Ganges, could be corrected and the river constrained to flow in a straight channel in line with the guide-banks by means of suitable permeable training works, whilst the spill from the Ganges which threatened the rear of the position could be closed. The Author was quite correct in asserting that the guide-bank system did not afford a permanent solution for the bridging of a river such as the Kosi; in fact, for such a river, in the zone where it was building up its bed, there could be no real permanence. Nevertheless, the judicious use of permeable training works could correct local defects, and could help to maintain a straight clean bed and prevent the formation of bends and loops, which were generally the preliminaries to a break-away; they could thus serve to postpone for years or for decades the day when the river would change its course.

Unfortunately it was not possible to refer to any river in Argentina comparable in size to those mentioned in the Paper, as a case where permeable training works had been carried out. The scale on which the work was done, however, although it might affect fundamentally the method of execution and the cost, did not alter the physical principles on which the underlying theory was based. In fact, the smaller Argentine rivers might almost be regarded as models of the larger Indian rivers. The only essential requirement for the application of the principle of permeable training works was that there should be a current laden with silt.

The simple type of training works used in Argentina and described above should prove equally valuable in India for rivers up to a certain size; for the larger rivers, or for those with permanent water, other methods would have to be devised, depending on local conditions and available materials. A system used in the United States, and aptly called

"retards", which was based on the use of whole trees suitably anchored, should prove effective. By means of suitable equipment, hurdles or cages built with Lombardy poplars, or frames of wood or metal covered with brushwood, could be placed athwart the current and anchored. Probably some experimenting would have to be done to determine the most suitable design. An advantage of "retards" or similar devices was that they could be laid at any stage of the river, and could be employed to protect a threatened position while the flood was in progress.

Mr. F. R. Morgan observed that the paragraphs on pp. 168 and 169 § dealing with the travel down country of the serpentine course contained or implied so much that was of great importance to the understanding of the behaviour of alluvial rivers in India, that some elaboration of their contents was desirable.

It was probably correct to say that the channels of all rivers that rose in the Himalayas and flowed across the plains of upper India travelled continuously down country, and that it was that "procession", which was a product of the parched nature of the country in the hot weather, and the very great difference in the volume of water carried by a river in the dry season and in the rains, which was mainly responsible for the occasional apparently freakish and unforeseeable behaviour of some of the alluvial rivers.

The ratio of bend to chord (or of a length of tortuous channel to the length of its main axis) was influenced by the following factors: the volume and consequent velocity of the dry-season discharge; the nature of the alluvium; and the general fall of the country or of the *khadir* or flood-bed. Alterations in any of those factors, except in so far as they compensated one another, would alter the bend-chord ratio. Thus the ratio varied with the loss of water on the way by absorption and evaporation, with the reduction of volume by the taking-off of channels or branches, with the addition of water by tributaries, with the changing of the general surface-slope of the *khadir*, and with the variation of texture and composition of the alluvium.

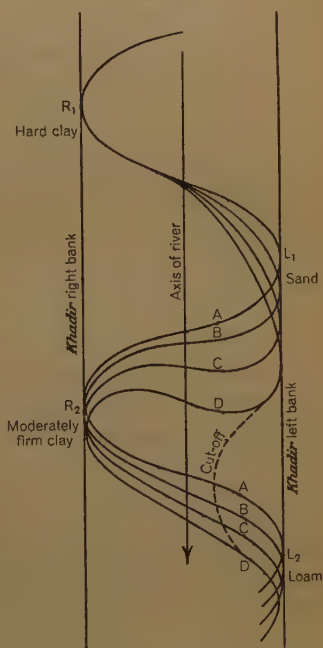
It was easy to see that in practice the hypothetical case of *Fig. 9* (p. 168 §) could not, except perhaps in a river on a small scale, occur in a river that had a *khadir*. There was reason to believe that something like it did occur in the probably geologically young streams that were crossed by the Kalabagh Bannu Railway on the North West frontier of India. Those streams had deep serpentine beds, dry for the most part of the year, but carrying occasional torrents in the rains. Before man denuded the hills of vegetation the run-off, it would seem, was such as to obviate short-lived torrents and the overflowing of the banks. Now, flooding took place across the crossovers joining right- and left-hand bends, with disastrous results to the railway. The bridges on the railway were

designed with pier abutments, so as to be capable of lengthening or shortening at either end, apparently in anticipation of the "procession" down country of the stream channels. The *khadirs* of those streams were in process of formation, but since the streams had become purely fast-flowing flood passages their *khadirs* would not be true *khadirs*, and the guide-bank principle of river-training would not apply to them. Steepening of the slope by the cutting through of the cross-overs had resulted in recession of the beds, and except where recession had already occurred foundations had to be taken sufficiently deep to allow for it.

Although the hypothetical case was nearly unthinkable, it might be utilized to illustrate the action of an alluvial river in certain known conditions. Let the river be assumed to consist of bends of equal arcs, chords, and versines as in *Fig. 9*. The portion illustrated in *Fig. 22* contained four bends, beginning at the top with a right-hand bend and ending at the bottom with a left-hand bend. Let the successive right-hand bends be termed R 1 and R 2, and the successive left-hand ones L 1 and L 2. Let R 1 be in inerodible clay, L 1 in friable sand, R 2 in moderately firm clay, and L 2 in loam, less erodible than crumbling sand, but more erodible than moderately firm clay. The arrow indicated the main axis. With the passage of years R 1 remained unchanged, L 1 moved very considerably, R 2 moved only moderately, and L 2 moved somewhat more, down country. The river would then have altered its shape progressively as illustrated, whilst conditions would then have been set up between loops L 1 and L 2 that favoured the early creation of a cut-off, so that one might be expected to occur.

In any alluvial river, however, cut-offs had occurred again and again, with bends of different arcs, chords, and versines, and the *khadir* was interspersed with old abandoned loops, in differing stages of siltation. Those abandoned loops, in favourable circumstances, offered relatively easy passage for flood water and facilitated the early formation of cut-offs. That was to say, the formation of a cut-off did not ordinarily wait upon the excessive distortion of a bend. When a cut-off occurred the river shortened its length and therefore steepened its slope and speeded up its current, so that erosion became intensified in portions of its channel,

Fig. 22.



and lengthening by embayment proceeded apace until suitable length and slope were attained.

As already stated, the successive loops or bends of a river were not equal regarding arcs, chords, or versines. If they were consistently equal the *khadir* banks would always be tangential to the loops, which was far from being the case. Whilst some loops adjoined the *khadir* banks others were at varying distances from them. The existence of a *khadir* bank far outside a loop was an indication that a loop had once been there. In many cases the "procession" down country of a loop of a river past any one spot that might be used for reference was normally a matter of many years, say a third of a century. Since successive loops were not equal the threat to some particular locality in the *khadir*, that might happen to be beyond the reach of smaller loops, might not occur with the passage of many loops, and might not therefore occur more than once in a century or so. If the *khadir* bank on the side of the spot under consideration were originally scooped out during the passage of a loop of very exceptional versine, it might be that the spot referred to would be safe for ever; that was to say, that the river would never again cut down the country in the *khadir* in this vicinity. On the other hand, if an exceptional loop of larger versine than any that had been in the *khadir* before developed up country and worked down, a new limit to the *khadir* might be created.

If all that were correct, then, taken in time, the threat to any very long-established place in the *khadir* or on the river bank could be forecast years ahead, when gradual deflexion of the stream, to protect the place, could very often be effected. Such threats were usually not seen until it was too late, however, and much money was wasted in a belated and often hopeless battle. The reason was that the river (often due to financial limitations) was studied only in the immediate vicinity, or for a very few miles up- and downstream, of the work or place concerned, and little or nothing was learned from the study. Knowledge of what was happening for several loops upstream of the part to be protected was essential to a proper forecast of the course a *khadir* river might be expected to take near any one place, and to the action to be taken for safety.

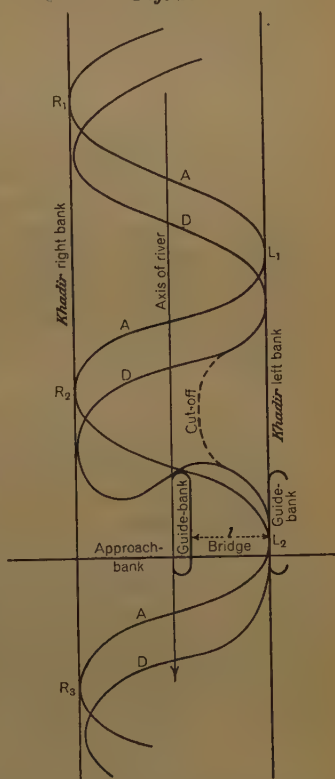
Since the left- and right-bank limits of the *khadir* were the creation of the largest left- and right-hand bends, it was clear that the whole *khadir* bed was not essential to the river as a flood channel; consequently where the river was to be crossed by a bridge it was easy to see that the whole of its *khadir* need not be bridged for passing flood water. Where the winter channel had meandered the most, the proportion of the *khadir* to be bridged would be least. Obviously, however, the river had to be induced to pass through the portion spanned by the bridge, and to be prevented from attacking the portion dammed by the approach bank. That was usually effected by Bell bunds. Usually only one bank at a time, of the two ordinarily provided, acted as a guide-bank, guiding the water through the opening of the bridge; the other acted as a limb, holding

the serpentine waterway of the river at arm's length and preventing its coils from reaching and attacking the side of the approach-bank. An attack on a bridge was really the approach and passage of a loop on one side, to be followed, at some later date (should the convolutions of the river be nearly equal on both sides of the axis), by a similar attack on the other guide bank, by a loop on the other side, and by a change-over of the functions of the two guide-banks.

Let the hypothetical case be taken of a river of consecutive equal and opposite bends with tangential *khadir* banks, which connoted a *khadir* bed of uniform texture and resistance to erosion, and the absence of depressions favouring the creation of cut-offs. Referring to *Fig. 23*, the bends were denoted by R 1, L 1, R 2, L 2, going on to R 3, L 3, and so on. L 2 might be selected for the crossing, and the bridge might be located far enough down to permit the guide-banks, of length l , say, to be built in the dry. The left guide-bank might be located on the left edge of the *khadir*. The "procession" of the channel proceeded year after year, as already suggested, but was opposed at the crossover R 2, L 2, by the right guide-bank. Embayment took place between the right guide-bank and the bank R 2, R 3, and conditions were set up favourable to the creation of a cut-off; the latter would occur somewhere between the main axis and the bank from L 1 to L 2. The river was thereby shortened and an increase in its activity occurred, so that accelerated erosion probably took place just below L 1, and an embayment formed there and went on increasing until a balance was attained. The "procession" of the

channel meanwhile continued and, in the course of time, bend L 1, now with a larger versine than at the outset, approached the left guide-bank, which was in due course attacked. That guide-bank was, however, far from the main axis of the river, and the attack on it and the embayment behind it would be relatively slight. In due course bend L 1 would pass through the bridge and the next attack would come from the loop that had taken the place of R 1, and would be on the right guide-bank. In an ordinary Indian river, where cut-offs might develop anywhere upstream of the bridge, that regular alternating left and right attack might not

Fig. 23.



occur, for a loop might be short-circuited by a cut-off so that it passed without appreciable attack through the bridge; the next serious attack might then come from the same direction as the last one.

Leaving that hypothetical case, it might be instructive to look into the actual case of the Ganges above and below the Hardinge bridge, illustrated for some 60 miles of its length in *Fig. 13* (p. 187 §). The main axis, from mid-channel near Sarda to mid-channel at the lower end, below Pabna, appeared to lie almost directly north-west and south-east. Working down from Sarda, the river, some 10 miles below, entered a great bight. The foot or L-shape of that bight was probably acquired when it was drawn upon in full force by the Jalangi and Mathabanga rivers, before they became dying rivers. The sharp-nosed projection at Raita on the lower end of the bight suggested the existence of very resistant material, whilst the flattening of the bank near Sara and the narrowness of the *khadir* in its vicinity was evidence of somewhat similar resistant material. Below Paksey the channel apparently lay in easily erodible alluvium, although two capes, one on each bank, some 8 miles along the main axis below Paksey, suggested local resistance to "procession", and, therefore, firmness. As with the two rivers above Raita, the "draw" of the Gorai river opposite Pabna had influenced the shape of the bight in which it took off.

Fig. 13 showed the course of the river in 1909 and 1780. What occurred above Sarda was not shown, so it was not possible to say what the influence of the direction of approach of the river near Sarda would be. The 1780 channel in the foot-shaped bight, as its shape and proximity to the *khadir* indicated, was, as already stated, probably greatly influenced by the "draw" of the Jalangi and Mathabanga rivers; the 1909 course could not be so affected, as those rivers had then become moribund, so that the development of the 1909 course directly southward was not to be expected. If the direction of the main axis influenced the direction of development of the channel, as it should, the stream would press steadily nearer and nearer towards Raita, carrying more water and probably having greater force than in 1780; that pressure would push the next, that was to say, the left-hand or Lalpur, bend (Figs. 14, 15, 16, 17, Plate 2, facing p. 224 §) north and east into and above Sara, where it would gradually flatten out till it lay along the Sara bank. That would ordinarily take several years. Further development at Sara would depend on what occurred above Raita, and that in turn, to a greater or less extent, by what occurred above Sarda. As long, however, as the loop above Raita continued to endeavour to pass downwards (its direction influenced by the direction of the main axis), the attack on Sara would continue; moreover, in the process of adjusting its length, the river below Sara would gradually work over to the right bank, and an attack on the right

bank and guide-bank would begin, and would continue until changes took place upstream to alter the line of attack. If embayment above Sara developed (as it had done), the attack on the right bank would occur higher up the river and the situation would be menacing for a long time. Sooner or later, it would appear, the right guide-bank would have to be lengthened. If the southward-pointing peninsula on which Sara and Paksey stood were removed artificially or by the action of the river, the bridge would be left high and dry by the river, for, judging by *Fig. 13* alone, the bight above Sara would merge with that below it, on the same bank. Consequently the fortification of the Sara-Paksey peninsula was advisable.

It was financially sound not to make the guide-banks the full length finally necessary, at the time of their construction, for the threats indicated above could not develop for many years and the lock-up of capital postponed would permit the saved capital to double itself. Nevertheless, early and better understanding of what the river might have been expected to do would no doubt have enabled the engineers in charge to take action earlier and more wisely, although it was not likely that the failure of the right guide-bank would have been obviated, for that failure, it might be accepted, resulted from inherent constitutional defects which were unsuspected, and which only failure under a very severe attack could bring to light.

Professor F. G. Royal-Dawson pointed out that the remedial measures taken in 1898 at the Empress bridge over the Sutlej had established the efficacy of an impregnable and suitably curved head as an essential feature in guide-bank design. The general question of the most suitable distance of such head from the bridge-site had, however, formed a subject of discussion. He fully agreed with the Author's view regarding the vital necessity of maintaining the Sara head as a key position. The prime cause of all the recent trouble was the collapse of that head in 1931, causing an obstruction which disturbed the smooth regime of the river. The reconstruction of the head in a slightly retired position and the provision of the Damukdia revetment on the right bank were necessitated by the altered conditions. The failure of the right guide-bank in 1933 was probably an after-effect of the disturbed regime initiated in 1931, whereby that bank became subjected to a degree of scouring action from which it had hitherto been immune. Briefly, he thought that the final remedial measures should be as proposed by the Author, treating the Sara and Damukdia positions as the impregnable heads of extended guide-banks protecting the bridge, with Raita as a vital pivot point farther upstream.

He had not had an opportunity of reading the Committee's report of model-experiments, but scale models were necessarily only of limited value, for they could not reproduce actual conditions, even if the same elements were used. If the scale of a model were $1/n$, current movements

of loop formation on encountering such fixed positions as the Raita point near H on the right bank, the Sara head near K on the left bank, and the two guide-banks at the bridge itself. He had plotted in two positions a typical S-curve in which the loop was 1.75 times the chord. The first position CDEF represented diagrammatically the main course of the river during and after the construction of the bridge, and the second, GHJK, a possible future development as suggested in an incipient stage in Figs. 15-17, Plate 2 (facing p. 224 §). West of the points C and G the tails of the S-curves were necessarily distorted to fit in with the axial change of direction. Eastwards of those points, however, the curves were true to type. From that it would appear that the encroachment on the Lalpur bight between E and F had now reached its limit, and that as the future alignment GH west of Raita point became more developed, the forward part of the loop, HJK, would ease the situation. Another point suggested by the diagram was that a lag occurred in the development of the forward part of an S-curve; thus, although the portion CD of the first S-curve had been established in 1915, the encroachments on the Lalpur bight between E and F did not begin to manifest themselves till 1925. Arguing on that assumption, the portion GH of the second S-curve would probably have to be established some time before the forward position HJK would be reached, and so on. The lie of the permanent banks also suggested that an anti-clockwise movement of loop G (due to creep) pivoting on Raita point would fit in with an S-curve occupying the western portion of Lalpur bight, which position would be even more favourable to the bridge.

Mr. C. Graham Smith observed that, although the general behaviour of aprons was much the same, in particular cases it varied. That was not surprising when it was remembered that they were laid on material that was anything but uniform. The soil might grade from clay and *kunkur* to fine impalpable silt. To illustrate the great difference, two examples might be cited of bridges built in alluvium. The first was that six 10-foot diameter wells of the Kursela Nala bridge (twenty-one spans of 40-foot girders) had been sunk to the full project depth of about 40 feet below ground-level (32 feet below low-water level) without ever having had a dredger put in them: they had been sunk by bailing with a "mote" (buffalo skin). Those wells had had to be pushed down to lower levels to make them safe, but the first 40 feet of sinking had been through a kind of quicksand. The remaining wells had been sunk through ordinary sand in the normal way. The other example was at the Inchcape bridge at Manjhi Ghat (eighteen spans of 200-foot girders). The average sinking of one of the wells throughout one whole working season had been a little over $\frac{1}{8}$ inch per day. Another well at a certain stage had a dredging hole as deep as 28 feet below the well-curb. In that case the soil was a very

stiff leather-like clay which was most difficult to under-cut below the steining. Both examples were in the Gangetic plain, and served to show what great differences there could be. With such varieties, it was only to be expected that all aprons would not behave alike.

The weight of the apron was no great help in making the under-water cliff cave in. Assuming that the weight of rock was 180 lb. per cubic foot, with 40 per cent. of voids, the effective weight would be 108 lb. Deducting the weight of water, at 62 lb. per cubic foot, the net available under-water weight was only 46 lb. per cubic foot. If, however, the voids were filled with silt and sand, say, at 100 lb. per cubic foot, then: 60 per cent. rock at 180 lb. = 108 lb.; 40 per cent. sand and silt at 100 lb. = 40 lb., or a total of 148 lb. per cubic foot. Deducting the weight of water, the net available under-water weight was 76 lb. per cubic foot. That was 30 lb. more than in the previous case, but it was not very much. The apron itself might have a certain amount of inherent rigidity, as during construction the interstices between the larger lumps of rock were filled with smaller pieces, and all those crevices in the course of time would get filled with sand and silt, making the apron a more or less solid mass, and tending to give it strength against bending.

On p. 153 § it was stated that "It is well-known that deep-seated slips usually take place with a falling river . . ." That was true in regard to both guide-banks and cliffs, both above and below water-level: the reason might lie in the fact that they were under hydrostatic pressure, and that the river, in falling, upset equilibrium.

The Bagaha bridge was designed for fifteen spans of 150-foot girders, but it was actually built as thirteen spans of 150 feet, one span of 200 feet, and one span of 100 feet, owing to the scouring out of well No. 4 during the course of construction in the floods of 1910. That well was reported to have been sunk to 60 feet below low-water level (with 20 feet of sinking remaining to be completed), when it was lost. That necessitated the sinking of a new No. 4 well in a different place, and accounted for the two odd spans. The approach to the temporary bridge as first erected took off from the old Chhitauni Ghat station, passed through the right upstream guide-bank, and curved to the bridge, which was upstream of, and parallel to, the permanent structure. The curve was on a sand bank and was heavily pitched on both sides with block *kunkur* and boulders. That pitching was never completely removed before the floods of 1910. That heavily-pitched approach might have formed a serious obstruction to the river when it came down in flood, and thus caused a deflexion of the water towards well No. 4 and so set up a dangerous eddy in its vicinity. One afternoon the well was seen to tilt over downstream, and gradually, and quite silently, cant more and more until it finally disappeared below water in a deep hole. In the following season well No. 15 was commenced,

but it was not sunk to full depth before the floods of 1911; in view, therefore, of the loss of well No. 4, the uncompleted well No. 15, adjacent to the east abutment, was very heavily pitched with boulders and block *kunkur* before the 1911 floods, and that pitching caused a lot of trouble in completing the sinking of the well in the following working season. It therefore seemed possible that that great mass of *kunkur* and boulders might have had some influence and bearing on the loss of well No. 14 when the river breached the right upstream guide-bank and set diagonally across to the left (east) bank.

It was mentioned (p. 147 §) that the right upstream guide-bank failed by percolation at the junction with the old temporary head, where a lot of *kunkur* and sausage protection had been thrown in. Mr. Smith had no recollection that there ever was a temporary head. He believed that the guide-bank was taken out to its full length of 2,000 feet in the 1909-1910 season, the season in which the pile bridge (already mentioned) passed through it. It was possible that that same old pitched pile-bridge approach which contributed to the loss of well No. 4 might have acted as a submerged weir, thus causing the breach in the guide bank. He was not aware of the conditions of the river when the breach occurred. If it took place on a falling river, it certainly was not caused through over-topping of the guide-bank, although he understood that floods had been registered at a height above formation-level of the guide-banks. Did not such a flood once scour behind the left abutment? It might have been caused by eddy-action set up by the nose of the guide-bank, the apron being too narrow to protect it. The apron was originally laid with a width of 40 feet and was 4 feet thick. The 1911 floods cut into the nose and the apron downstream, and caused a 400-foot length of slip in apron and bank. The apron was widened over a length of about 300 feet to 65 feet in 1912. That width was still insufficient, but he did not know whether or not it was made wider subsequently.

In the Paper it was observed that the project was based on a fall of 1.35 foot per mile of unobstructed river. His recollection was that the fall was about 18 inches per mile at Bagaha. In a distance of some 130 miles from Bagaha to the Ganges, the fall was from 295 feet to 177 feet; namely, 118 feet, or an average of rather less than 1 foot per mile. If it were double the average at Bagaha the fall would be less than 2 feet per mile. He therefore agreed that the fall was bound to be well below the figure of 3.3 feet per mile mentioned in the Paper; it was more likely to be about half that amount.

The Inchcape bridge (eighteen spans of 200-foot girders) was built across the Gogra at Manjhi Ghat. Like the Bagaha bridge, there was an "old hard bank" and no left guide-bank. The right upstream guide-bank was 3,000 feet long, the end 600 feet being on a curve of 1,000 feet radius.

The guide-bank was at right angles to the bridge. In 1909 or 1910 that guide-bank was breached downstream of the head, caused by the approach of the river diagonally from right to left creating an eddy into which the apron and guide-bank slipped. The slip started as a small one, but it was sufficient to permit the river to charge through. The breach widened at the rate of 100 feet per hour for 4 hours, when it was arrested. A new temporary nose was made and held. That temporary head then caused another eddy into which another portion of the apron and guide-bank slipped, thus creating a second breach. It was feared that the process might be repeated and that the right (west) abutment would be scoured out, taking with it the land span that had already been erected, but fortunately that did not happen. Those two accidents proved that the apron was not able to perform the duties expected of it. The scour-hole at the original head was measured at 96 feet from about three-quarter flood, or about 80 feet below low-water level. All the wells that were founded in sand were carried down to 100 feet below low-water level. At Manjhi Ghat the river slope was about 9 inches per mile.

On p. 167 § it was stated that guide-banks could only be built on dry land. Actually the left upstream guide-bank of the Kosi bridge was built across the river in order to divert it through the bridge. The first four wells put down were intended to be in the middle of the bridge, but the river gradually crept eastwards, and all subsequent wells were put down on that side, the original four wells thus forming the west (or right) end of the bridge. When all the wells were completed, the river was flowing behind the east abutment, and measures had to be taken to force it through the bridge. That was done by constructing the guide-bank from the upstream end. A pile bridge had to be built across the river and pitching was dumped into the water from ballast trains above. The water-way was thus closed and the river diverted down a channel that had been previously prepared to receive it. The piles of that old bridge were still embedded in the guide-bank. In 1897 the Ganges was some miles away from the Kosi bridge site, but in 1903 the Ganges steamers were working close to the "Ganges West Fender"; in 1913, however, the Ganges had receded many miles, although the spill might still exist and involve a threat to the Katareah approach. He had been told that during the course of construction, the railway administration had actually contemplated abandoning the bridge site in favour of one some miles upstream.

At Bagaha 8 feet of bed-scour had been observed to take place in 2 hours under somewhat abnormal circumstances, whilst at Manjhi Ghat the river could silt some 5 feet a day, and a pile bridge of one hundred spans of 20-foot girders had one span successively silt up every day for most of a month.

The Author observed on p. 163 § that no objection should be taken to

the cost of the steel span exceeding the cost of the pier by a reasonable amount. Many bridges had been designed in which the cost of girders was approximately equal to that of the piers and foundations. In view, however, of the heavy cost of maintenance, and of pitching around wells, it would appear to be cheaper in ultimate cost for the same length of bridge—having, incidentally, a less obstructed waterway—to permit some increase in span in order to reduce the number of wells. The matter was, however, a question of economics.

The Author advocated a non-alluvial clay or ballast soling on the guide-bank slope under the hand-set stone pitching. Had he ever considered using burnt-clay (brick) slabs instead, say, 12 inches by 12 inches by 3 inches, placed close-set on the flat? The Author was quite right in his contention (p. 183 §) that the guide-bank apron should be quite independent of the pitching round the adjacent pier. That would tend to prevent the formation of a submerged weir and unnecessary scour.

The Tables on pp. 175 and 177 § indicated an immense increase in cost of training and protection, but in the end it was cheaper to do the work thoroughly and to make it secure than to incur the great expense of making good damages and maintaining works that were not sufficiently robust.

Mr. W. L. C. Trench observed that it was evident that much systematic investigation and research were still necessary before any principles likely to gain general acceptance could be laid down.

In his description of the Curzon bridge the Author stated that "it would in all cases be necessary to ascertain the ratio of bend to chord at which cut-offs occur in the river it is proposed to bridge." That implied that there was a fairly constant ratio in any particular river. That view was, indeed, expressed in so many words, in his later remarks on cut-offs. No data justifying that general proposition were, however, given, and it was of extremely doubtful applicability to the lower Indus in Sind.

A cut-off could occur in two ways. The first, and more usual, way was when the flood spilling across the chord developed a scouring velocity, and cut a new channel, or, more often, reopened an old channel. The second way occurred when erosion in both curves forming the neck continued until the upstream and downstream erosions met, or nearly met, before the neck gave way. In the latter case the horseshoe curve shown in *Figs. 8 (b)* (p. 168 §) occurred. That condition was described by the Author as belonging to a dying river. It had, however, no necessary connexion with a dying river, and it was not uncommonly found in the Indus in lower Sind, where the first type of cut-off also occurred. In fact, the only connexion was that rivers tended to die if their slopes were too flat, whilst, for reasons given below, horseshoe curves tended to occur where the slope of the country through which the river flowed was flat.

The conditions in which horseshoe curves were found in flat country were : (1) when the material composing the neck was hard compared to the soil at the arc, and/or (2) where the level of the ground at the neck was high, so that little water passed over the neck, even at flood times. The effect of those conditions was, of course, that the arc had a longer time in which to lengthen out. A case of that kind occurred on the Indus in the Karachi district, about 1927, where the chord had a length of only about 1 furlong, whilst the arc was several miles long. There had also been other examples. The reason why that type of tortuosity was found in flat country was, according to some authorities, because where the slope of the country, and therefore the maximum slope of the river, was flat, the average velocity was low—and, therefore, the increment of erosive force, due to the centrifugal action of the current at the arc, was great—compared to the erosive force of the current.

The reason why a cut-off was likely to occur in flood conditions was not that a river in flood preferred a straight course, as stated by the Author, but because it was only then that water could pass across the chord, or, where a secondary channel or depression was already available, could pass in sufficient quantity to develop a scouring velocity. Whether or not a scouring velocity developed depended (a) upon the fall available across the chord, and (b) upon the discharge, because the greater the discharge the less the fall required to cause scour.

The fall available in any chord could be increased in two ways : (1) by the arc lengthening itself by erosion, and (2) by an alteration in the "set" of the river, generally at the top end of the chord. In the first case the greater the distance round the arc, the greater would be the fall available across the chord. That was due to the fact that the river had to maintain a minimum slope to carry the discharge and silt burden, and that if it became too flat the river would tend to build itself up above the curve, thus increasing the available slope. (Conversely, when a cut-off occurred a retrogression of levels took place, which might persist for many miles upstream.) In the second case, when a river flowed in a curve in plan, the water flowing round the outer, or convex edge, was raised above the water-level of the inner edge. That difference of level was generally reckoned to be from 1 foot to $1\frac{1}{2}$ foot on the Indus. If, therefore, the "set" of the river were towards the head of an available chord channel, that channel would be more likely to re-open and to become the main channel of the river, owing to the extra head that became available in that way.

Where there was already a fairly well defined channel across a chord, the discharge for any available difference of level would clearly be greater than where no such channel existed. In such circumstances a cut-off was liable to occur earlier, and therefore at a smaller ratio, than where there was no channel or depression. Moreover, although, at the same place in any river, the same conditions might tend to recur after an interval of years, it was unsafe to assume that, because the river had cut-off at a

certain approximate ratio at one or more places, it would therefore cut-off at the same ratio in another place, possibly in completely different conditions.

The Author stated that the serpentine track of a river running between fixed banks tended to travel continuously, though slowly, down country. It would be of interest to have some data on which the Author based his view. Some authorities were of opinion that such serpentine channels tended, on the contrary, to move upstream. Mr. Trench did not think that the available data was conclusive either way. He would, however, call attention to the curves on the Ganges just below the Hardinge bridge site, between the years 1855 and 1875 (Plate XIII of the Hardinge Bridge Committee Report). It was true that there were no fixed banks at that site, in the sense used by the Author, but for 20 years the river was as stable, in that place, as if there had been such banks. It would be seen that, if anything, those curves had a tendency to move upstream.

The foregoing points were some of those on which the Author's principles did not seem to be of general applicability.

In reference to the Hardinge bridge, and to the report of the Committee thereon, the Author began with an enquiry into the causes of the breach on the 26th September, 1933, in the right guide-bank. Model-experiments made in Poona indicated that the breach was caused by toe-scour resulting from "diving-flow." As the other results of the model-experiments again did not support the Author's views, he was concerned to show that the experiments were untrustworthy in that respect also.

The difficulty in assigning to the Hardinge bridge case the more usual cause for the occurrence of a slip, namely scour along the toe of the protection, was that, at the material time, eddy action along the guide-bank either was absent or was insufficient to cause deep-seated scour. It was on record that from the 2nd to the 9th September swirls—that was to say, moving eddies—were observed along the bank, but that, after the change in direction of the current on the 9th, no serious eddies were observed (Hardinge Bridge Report, p. 22), and there was no mention of any swirls. The late Mr. B. L. Harvey therefore fell back on the theory that the local swirls and eddies had sucked the sandy material out of the bank, thus causing a collapse. Mr. Trench, however, never considered that the cause assigned was sufficient to produce the effect in the time available. Nor, apparently, was the Author quite satisfied with it, as he produced the additional, or alternative, theory that the collapse was caused by a breaking wave travelling down the bank once every 2 minutes.

In regard to that the only evidence available was that after the breach occurred a wave or surge was observed in the breach, which filled up the bay behind the breach to an additional height of 2 feet. That condition lasted for 12 hours, and was apparently due to some rhythmic alteration in the flow of the river. As there was no direct evidence that a breaking wave travelled down the guide-bank before the breach, nor

down the isolated head of the guide-bank after the breach (as would have been expected, at any rate for a short time thereafter, or until the river broke through at the back), the existence of the wave was only an inference from the fact that a surge was observed in the breach.

Mr. Trench had seen the damage done by wave-action on pitching near Hyderabad (Sind), in much the same conditions as existed as the Hardinge bridge. At the former place the monsoon winds blew strongly, and there was a length of pitched river bund open to the full force of the wind, with a "fetch" diagonally across the river of a mile or more. Waves 2 feet high were not uncommon there, and sometimes washed up on to the pitching, not every 2 minutes, but in rapid succession, for days at a time, giving cause for considerable anxiety. The principal danger was, however, the washing of the waves above the level of the pitching, which did not, or used not to, extend to the full height of the bund. The pitching was comparatively thin (not more than from 12 to 18 inches thick), yet the damage done extended only a few feet below the surface-level of the river, and consisted not in catastrophic slips, but in comparatively shallow subsidences.

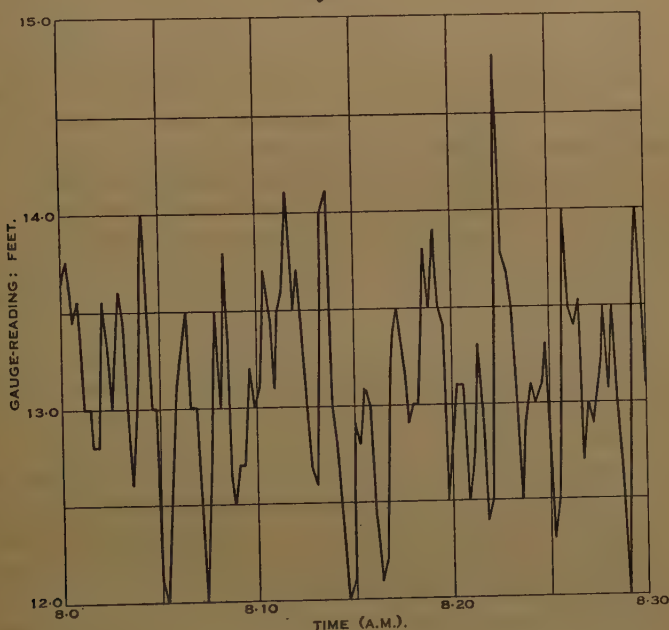
The waves conjectured by the Author had therefore to be rejected as a cause of the breach, because there was no evidence that they ever existed outside the breach; further, it was most improbable (it was, in Mr. Trench's opinion, impossible), that, if they had existed, they could have caused the damage alleged in the time available.

What probably happened was that there was a rhythmic alteration in the level of the river in the form of "breathing." Such breathing was a well-known phenomenon in the Indus at Sukkur. It varied with the stage of the river, and was particularly marked when the river was rising. The amplitude could amount to as much as from 2 to 3 feet, with a period of up to $3\frac{1}{2}$ minutes. That condition was allowed for in the orders of the Indus River Commission for the observation of gauge-readings, the repeated maximum during $\frac{1}{2}$ hour being taken as the gauge-reading of the day. *Fig. 25* showed an actual record of that breathing as it was on the 19th August, 1935. A similar rise and fall would lead quite naturally to a surge entering the breach, on the same principle as a tidal "bore", and no wave travelling down the face of the guide-bank would be required to produce it. The surge would eventually disappear when the breach got deeper, which explained why it only lasted 12 hours.

That suggested explanation at any rate covered the reported facts, and Mr. Trench did not assume, as the Author did, the existence of a breaking wave travelling down the guide-bank, which had not been reported, and which could not fail to have been seen had it existed. On the other hand, a rhythmic rise and fall of the river, over a period of 2 minutes, would not be in any way dangerous to a heavily-protected bank, and would certainly escape observation unless the gauges were being closely watched.

The Author, however, relied on two circumstances to differentiate the breach from the subsequent slip, and to justify his search for a different cause. One was that, at the site of the breach, 400 cubic feet of stone per foot run had been added to the toe of the apron; and the second was

Fig. 25.



In regard to the extra stone, it was, of course, impossible to say to what extent it did, or did not, delay the breach. On the other hand, the reason why that particular breach occurred on the rising freshet was clear from the diary of events (Appendix I, p. 22, of the Report). It would be seen that between the 9th and the 16th September there was a marked change in the direction of the flow of the main stream: up to the 9th it impinged on to the Damukdia guide-bank, whilst after the 16th it impinged directly on to the right guide-bank at chainage 20, the site of the breach. As already mentioned, there were no serious eddies reported

there, but the main stream was bound to have been sharply curved in plan, in order, after impinging on the bank, to flow along it. That would inevitably lead to diving flow sufficient to cause dangerous and deep-seated scour.

Neither of the objections put forward by the Author were therefore sufficient to preclude the breach having been caused by toe-scour, and as there was, as pointed out, no evidence of any other sufficient cause, it had to be concluded, contrary to the Author's opinion, that it was caused by toe-scour.

The Poona model-experiments were thus confirmed to that extent.

In regard to diving flow, the existence of that form of flow had long been known, but it was only the recent growth of the experimental method of investigating hydraulic problems, especially those concerned with the transportation of silt, that had emphasized its importance. Hence the original suggestions regarding the cause of the breach left that important, and sometimes destructive, form of flow out of account.

The convergence of the guide-banks of a bridge upstream did not seem to be a matter on which railway engineers were as much agreed as the Author suggested. The reasons for it were, in the Author's words, "to reduce obliquity of current at the piers, and to hinder the formation of sandbanks at the throat." In regard to the first reason, it was not easy to see how guide-banks oblique to the bridge-line could promote a more normal approach than guide-banks at right angles to the bridge. In fact, in the plan of oblique guide-banks in Bell's Paper, printed as an Appendix to the Author's Paper, it would be seen that none of the ideal streamlines therein drawn was parallel to the piers, except the centre one, which was what would be expected. In regard to the formation of sandbanks, it was not clear how any narrowing in excess of the combined thickness of the piers, which would result in leaving a waterway less than that through the bridge, would be advantageous.

In the case of the Hardinge bridge, however, any further protective work there was conditioned by the existence of the Damukdia protection-bank on the one side and the Sara protection-bank, and the amount of the embayment above it, on the other side, whilst any extension of the existing guide-banks had necessarily to be on the same parallel alignment as the short banks already in existence. It should be noted that, whilst the Author's own proposals, as shown in Fig. 17, Plate 2 (facing p. 224 §), involved long divergent protection, the recommendations of the Committee were comparatively orthodox in favouring guide-banks that were at least parallel. It was not clear how a contracted throat could now be applied to that site, or how, in view of their recommendations, the alleged tolerance of the canal engineer for the splayed guide-bank could have had any influence on the Committee's recommendations. Neither was it

clear why the problem of passing a meandering river over a weir should necessarily be, in the Author's opinion, so much easier than passing it under a bridge, nor how that question was in any way relevant to the matter in hand.

In regard to the conditions found by the Committee at Sara, and their attitude to the Sara protection-bank, the Author stated that, having laid down a tentative line, indicating what would constitute a smooth approach to the bridge, they called the area which projected beyond that line a "protuberance", and blamed the untoward action of the river on the guide-bank on that protuberance, which, in the Author's view, was actually non-existent. That statement was entirely incorrect, as was borne out by the Report itself. To make that clear it was necessary to refer to the procedure of the Committee.

On arrival at the bridge site on the 27th August, 1934, the Committee, of which Mr. Trench had the honour of being a member, were fortunate in finding a very high flood running. They proceeded to examine carefully the various important sites at and above the bridge. At Sara they found the main stream of the river close under the Lalpur bight. When it reached the Sara protection-bank, so far from smooth flow having been established in the previous year, as stated by the Author, there was violent eddy-action off the protection-bank, resulting in a scour-hole 205 feet deep, and the deflexion of the current diagonally across the river. Not only so, but above the protection, as Mr. Trench himself observed, there was a distinct, though slow, reverse current under the bank. The track of the main current and the result of the eddy-action was clearly shown by the deep contours on the plan (Sheet I, part II, of the Report). The Author stated that a spur might be defined as "any solid projection from the river bank into running water, which is the cause of stationary eddies." In August 1934, the Sara site conformed accurately to that definition.

As mentioned, the Author stated that smooth-flow conditions had been established at the site in 1933, and that the conditions of flow, so far as the crossing of the current to the other side of the river was concerned, were merely "permitted" by the shape of the Sara site, and were not coerced. That was, however, not the case. In August 1934, the shape of the Sara site was still the active agent in forcing the main current across the river. The Author had not, Mr. Trench believed, had the advantage of seeing for himself the conditions at that place, in time of flood, during recent years. Had he done so, he would have probably been in as little doubt as were all the members of the Committee that the Sara protection-bank was still the main cause of the destructive action of the river on the right guide-bank, and that it did, in fact, act as a projection, protuberance, or spur in respect of the main current of the river.

As shown in paragraphs 7 (i) and (ii), on p. 28 of the Report, it was only after that thorough examination of the conditions at the site, that

any attempt was made by the Committee to see whether or not a modified alignment of the river-edge could be obtained, which would give smooth flow up to the bridge.

The tentative line had, in fact, nothing to do with the forming of the Committee's opinion in regard to the effect of the shape of the river bank on the main current. It was, rather, that very clear effect which suggested the possibility of such a line. It was soon found, however, to be impracticable as well as unnecessary, and it was only noted on the plan to keep the records complete. As, however, it bore no sort of relation to the final recommendations of the Committee, it was not clear why the Author should suggest that it continued to have some influence on the deliberations of the Committee.

The fundamental difference between the views of the Author and of the Committee was that the Author did not consider that it was the original abortive attempt to protect the Sara clay which had been the whole cause of the trouble. That clay had been scoured back very slowly for at least 200 years, but during that time it had always kept a straight alignment. There was no reason to anticipate that that action would not have gone on at the same slow pace, or to anticipate any danger therefrom within any period which could be foreseen, and for which it was necessary to provide. The clay was not a narrow band which could be quickly eroded away. Plate 8 of the Report showed that there was a considerable thickness of it for 2 miles or more behind Sara. As soon, however, as the current touched the protection on the clay the protection became a spur, with all the usual consequences of accelerated erosion above it.

The Author did not appear to be correct in stating that the position (presumably favourable to the reopening of the Damukdia channel) had now been lost. Actually that channel, even in the 1938-1939 season of lowest flow, was taking an appreciable percentage of the flow of the river. There was, therefore, every reason to anticipate an improvement in the channel during the next flood season. As mentioned above, and contrary to what was stated by the Author, smooth-flow conditions had not been established at Sara before 1934, at any rate in flood-conditions. Flow-conditions had now much improved, however, and the edge of the river was stated to be on a regular curve, although smooth-flow conditions had not yet been completely established.

The Author, in further reply to the Discussion and in reply to the Correspondence, would like to thank Sir Leopold Savile for appreciative remarks on the value of his Paper, and to endorse Sir Leopold's views on the desirability of the fullest investigation of every aspect of the problem of bridging one of the rivers of the type in question before the formulation of any definite scheme. There were, however, in those alluvial rivers important questions concerning maximum depths of scour about which the fullest investigation could supply no answer. The questions were, what was the depth below low water of the maximum scour which could occur,

in the particular river, alongside a guide-bank, (1) at the site of the bridge and (2) at the head of the guide-bank? The only information obtainable by investigation was the greatest depth of scour that could be found at a cutting bend in the alluvial sand deposit. That would be found after a high flood at about three-quarter stage on a falling river. It might be several years before there was a flood sufficiently high to obtain even that information. If there happened to be a semi-permanent or permanent river bank in the vicinity it might be a matter of 40 or 50 years before the river would be in a position to produce the maximum depth of scour alongside it. The depth of scour along a permanent bank would be greater than the depth at a semi-permanent bank, but although the depth at a permanent bank might be suitable for the guide-bank at the site of the bridge, it would not be sufficient for the head of the guide-bank. In nearly all cases, therefore, the guide-bank protective covering and the pier-foundations had to be designed for depths computed from the maximum depth to be found at a cutting bend of the river, as described. The depth of scour depended on the degree of fineness of the river-bed sand, the depth increasing with the fineness and increasing rapidly with the proportion of silt contained in the sand. The depth of scour also increased in some proportion with the magnitude of the river, as measured in cubic feet per second if not as measured by the total discharge during the 3 months of the flood season. It was in the belief that there was some connexion, in the rivers of northern India, between the fineness of the sand and the magnitude of the river, that he had offered his formulas, which were based on the magnitude of the rivers and embodied the experience of several examples. Confirmation was certainly desirable, and if any engineer in charge of guide-bank bridges, who was interested in the subject, could send him the data for each of his bridges, they would furnish a valuable check and the Author would be grateful. In the Author's view very valuable comparative results would be obtainable by model-experiments on the subject, and it was remarkable that although the flow of water in connexion with river-training had received a great deal of attention, no consideration had been given to the behaviour of varieties of sand, a subject which was equally important.

Dr. Herbert Chatley had asked a very pertinent question in inquiring under what circumstances the apron system of river-training was to be preferred to the mattress system. That could probably only be answered by an engineer who was equally conversant with both systems. If the inquiry were confined to river-training for railway bridges the question would be somewhat simplified. The apron acted perfectly where laid over alluvial sand, but the action was impeded by even thin beds of clay or other hard material. An apron designed on the Author's principles was automatically distributed with great evenness over a 1-in-2 slope by the action of the river. If in the course of development thin patches or complete gaps appeared, the exposed sand was removed by river scour and the

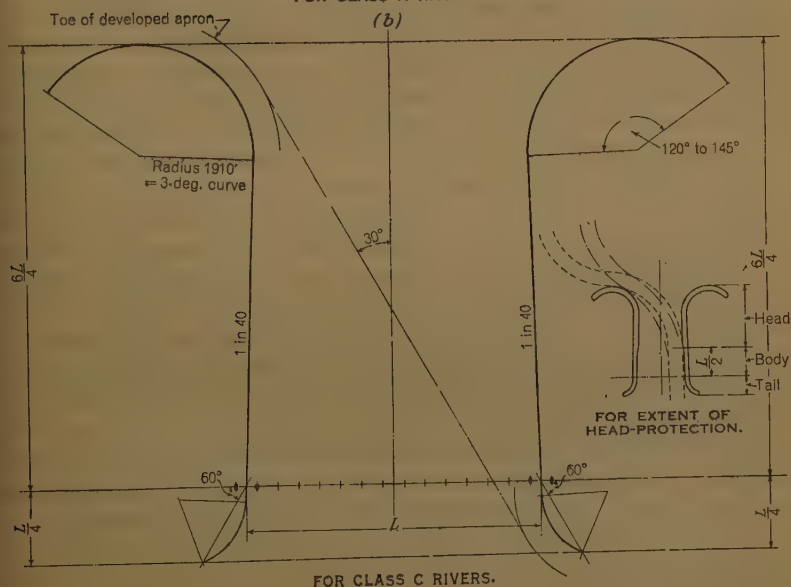
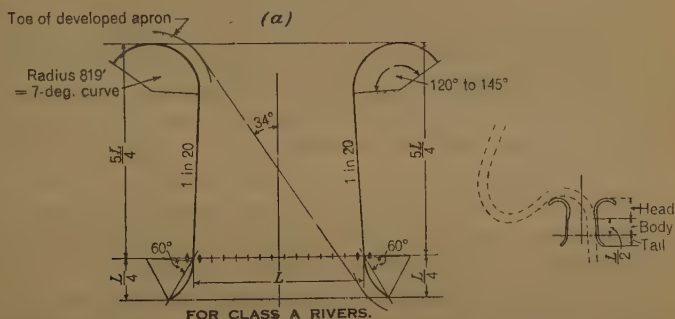
thin patches or complete gaps were filled by stone coming down from above. The apron laid on the flat provided a completely flexible covering for the resulting slope. A mattress could be laid on a prepared slope above water-level or it could be lowered on to a river bed prepared by the river, but the Author had had no experience of the behaviour, when undercut by the river, of a mattress laid on a river bank either parallel or at right angles to the river. It appeared to be extremely unlikely that it would be sufficiently flexible, or that the undercutting would be sufficiently even, to enable a mattress weighted with stone to carry down its load so as to provide a permanent covering for the slope. The real difficulty, however, in making use of the mattress in such guide-banks was due to the perishable character of the willow or bamboo components. The stone apron, on the other hand, had no such disability, and its great advantage was that it could be put in place in the course of a working season throughout the whole of the pair of guide-banks, provided that the site of the guide-banks had been so selected that the apron could be laid in the dry, before its position would be encroached upon by the river. In all such cases it was probable that the head of one guide-bank and the body and tail of the other would first come into use, and the apron of the rest of the guide-banks might remain untouched for a number of years. No matter, however, after what lapse of time it should come into action, the stone apron would provide a permanent protection for the slope, whereas any mattress, of willow, bamboo, or the like, would have perished. It would therefore seem probable that, for rivers running in recent alluvial plains formed wholly of light sandy deposits, the stone apron had no competitor, but whether or not such rivers were to be found outside northern India the Author was unable to say.

The ideal figure for a streamlined guide-bank was undoubtedly a curve of diminishing curvature for the head, continued as a body of slight curvature, followed by a tail consisting of a circular arc of greater curvature. The Damukdia guide-bank was designed on that pattern, but owing probably to difficulties in setting out, it was constructed with a short straight body, connected by a transition curve with a head consisting of a circular arc. To simplify the design of the converging guide-banks a straight body was adopted, with a head consisting of a circular arc of what was believed to be ample radius for each of the three classes of river A, B, and C, classified by magnitude. The scale of the protective covering designed for the head, which was greater than that required for the body, was made to overlap the tangent point by a large proportion of the straight. In such rivers it had been found that, when there was any considerable embayment, the main stream would leave the curved head, unless pressed into the guide-bank by water coming straight down the river in times of high flood.

The Author was highly gratified to learn from his remarks that the views of Lieutenant-Colonel Macrae so closely agreed on all points with those set forth in the Paper, particularly as he (the Author) knew of no one

better qualified, from his knowledge of both sides of the subject, to give an opinion. Colonel Macrae had remarked that in the Punjab, where the total span, L , for many bridges was about 2,000 feet, experience had shown that trouble developed in most cases where the upstream length of bund

Fig. 26.



was less than about 2,500 feet. It had long appeared to the Author that L was insufficient, and finding that confirmed on such good authority, he now proposed to increase the minimum upstream length of guide-banks for class A rivers from L to $1\frac{1}{4}L$ as shown in Figs. 26 (a), which was a revision of the original diagram (Figs. 7 (p. 164 §)). The criterion for the length of a flanking guide-bank was the permissible degree of maximum

obliquity of current at the piers. The Author had not previously visualized any means of delineating the maximum obliquity on the diagram, but seeing the depths scour could attain at guide-bank heads and what a narrow channel was required by the main stream in such circumstances, it now appeared that a line, drawn diagonally across from the head of one guide-bank to the tail of the other and tangential in each case to the curve of the toe of the fully developed apron as designed, would give, at its intersection with the axis of the bridge, the maximum degree of obliquity permissible. The degree of obliquity had also been shown on the diagram for class C rivers (*Figs. 26 (b)*). It would be noticed that the length of the tail in that diagram had been reduced to $\frac{1}{4}L$, the length which Colonel Macrae considered to be more than enough. The maximum permissible obliquity of current at piers by that construction had been found to be 34 degrees for class A rivers and 30 degrees for class C rivers respectively. At high flood it might be assumed that the river would run straight through the bridge, and it would be only at lower levels that that high degree of obliquity would be experienced. At the original Bell bund bridge over the Chenab at Sher Shah, after the head of the right guide-bank had been stabilized, the river took up a position on the diagonal between the right bank head and the left bank tail, and it was very desirable that the design of training works should be such as to avoid undue scour at piers with the river in that position. The purpose of the upstream convergence of the flanking guide-banks of a railway bridge was, (1) to reduce, so far as possible, obliquity of current at the piers; (2) in case of embayment at the back of a guide-bank, to protect the bridge approaches by bringing the river back to a direct course through the bridge as soon as possible by means of hastening the cut-off; (3) in case of embayment at the back of one guide-bank, to ensure, so far as possible, the river being turned by the upstream part of the other guide-bank, leaving the river free to pass quietly down through the bridge (as shown in the small diagram for head-protection accompanying *Figs. 26 (a)* and *(b)*); and (4) by "throating" the river between the guide-bank heads, to hinder or discourage the formation of sandbanks in a position which would cause afflux and consequent scour at the guide-bank heads—in other words, to invite the formation of a gorge. Moreover, it was a peculiarity of the rivers that, no matter to what extent the waterway at the bridge had been constricted, the main stream in flood time would be found to run as a rapid compact stream through the less rapidly moving flood-water. Furthermore, if that stream were in hard contact with one of the guide-banks in passing through the bridge, the width of the stream would be decreased and the depth of scour considerably increased. It was, therefore, important to avoid the concentration of the main stream in the angle between a guide-bank and the bridge, and long guide-banks converging upstream were advantageous in that respect. The demonstration of the thesis, that the length of the approach-bank protected by a guide-bank was proportional to the length

of the guide-bank, was not affected by the change of the minimum length of guide-bank from L to $1\frac{1}{4}L$ for class A rivers.

Many suggestions had been closely examined in the search for some reasonable explanation of the extraordinary action of the Committee in making the demolition of the Sara protection-bank the first step in remodeling the training works of the Hardinge bridge. An almost unnoticed remark, made by the Chairman of the Committee in the course of the discussion, appeared to offer yet another explanation. The remark read as follows: "At Sara, . . . detached fenders . . . intended to guide the river tended to aggravate matters and do more harm than good." It was evident from the context that the work described as a "detached fender" was the Sara protection-bank. Having mentally removed Sara and all its works from consideration, it was not surprising that he had found the flanking guide-banks of the bridge too short. The remodelling of the training works had then been easily disposed of by reverting to the two flanking guide-banks of upstream length equal to the width of the waterway, without any detached works upstream, as proposed 50 years ago by Bell for bridges over the Punjab rivers. It was then found that a retirement, by from $1\frac{1}{2}$ to 2 miles, of the whole of the main-line approach from Abdulpur to the bridge had been necessitated, with dam and sluices across the Ganges spill. The cost of that retirement was the price to be paid for the demolition of the Sara protection-bank, the commanding position of which had hitherto been able to ensure that any bend of the river above Sara would cut-off before the safety of the main line was threatened. The original misconception had consisted in mistaking the Sara protection-bank for a "detached fender." The Author shared the Chairman's dislike of detached works, which became a positive danger when out-flanked. The Sara protection-bank, however, so far from having been a detached work, was an integral part of the training works and it could never have been out-flanked. It was connected with the bridge by the main-line bank and the Sara siding, both of which were above high-flood level. The Sara protection-bank in fact formed the head of a "skeleton" or "interrupted" guide-bank connected with the left guide-bank. Moreover, the natural bank of the river between Sara and the bridge was high and well consolidated, and it had been raised above maximum high-flood level by means of a marginal bank about 3 feet in height. Similarly, although the distance from the bridge was greater, the Raita protection-bank was an integral part of the training works. The works at Raita and Sara had been named protection-banks by the Author to indicate that the positions they covered were to be held at all costs. The original right guide-bank was certainly not long enough to meet the conditions which had arisen at Sara, but with Sara no longer available to control the river, both of the flanking guide-banks proposed by the Committee had become relatively less sufficient in length.

Having come to the conclusion that the Sara protection-bank was

entirely harmful, the Committee had referred the question to the Officer in charge of the model-experiments, with the result that the subsequent experiments had been held to confirm the harmfulness of the Sara protection-bank and to show that the Damukdia guide-bank was quite as much to blame, and that both should be demolished. Those conclusions, however, could not be accepted as they appeared to have been based on the private views of the Officer in charge rather than to have found support in the experimental results. For example, in the Correspondence on the late Mr. B. L. Harvey's Paper on the restoration of the breach*, the Officer in charge had explained that opposing the direct force of flow ignored the first principles of river-control, and that the only way to control flow was by "coaxing" the river to swing in a large, natural curve, around a fixed guide bank." The subject of the model-experiments under discussion, however, was not river-control, but the design of guide-banks for railway bridges. With a pair of guide-banks, in the case of embayment of the main-stream at the back of one of them, it had usually been found to be the duty of the other to meet the main stream and to turn it down through the bridge. There was thus a direct conflict of opinion on a crucial point, for if the dictum of the Officer in charge were adopted it would mean the end of the whole structure of river-training for railway bridges. It was on that dictum that the Damukdia guide-bank had been condemned.

Returning again to the position at Sara, it appeared to the Author that the Committee, and subsequently the Officer in charge of the model-experiments, obsessed by the idea that Sara was a detached fender or at least a protuberance, blamed the so-called protuberance for the damage suffered by the right guide-bank, whereas an experienced independent observer would have recognized at once that the character of the attack on the head of the right guide-bank was relatively not more severe than the head of every guide-bank, sooner or later, would be called upon to meet, and that the damage suffered by the right guide-bank was due to the unstable nature of the sand of which the guide-bank consisted, for which the protective covering, disposed according to the ideas of the time, had proved to be quite insufficient. It appeared, indeed, that, but for the unsuspected existence under the right guide-bank of the "clay-patch" strata, the guide-bank would have been destroyed. It had been the Author's belief that at the exposed position in the open river at the site about $2\frac{1}{2}$ miles below Raita, no left guide-bank could be built which would withstand the force of the floods of a single season. Apart from other reasons, that had been decisive in favour of the site below Sara, and although he had since designed, in the Paper under discussion, a guide-bank covering more suited to the size of the river and the character of its sand, he would hesitate to trust to it alone in such a position as the site

* Correspondence on "The Restoration of the Breach in the Right Guide Bank of the Hardinge Bridge." Journal Inst. C.E., vol. 6 (1936-37), p. 325 (October 1937).

below Raita, and he would much prefer to shelter below some natural feature such as that at Sara which had been so lightly abandoned to destruction.

The Author was concerned to learn from the communication of the Director of Model Experiments, Poona (referred to above as the Officer in charge of the model-experiments), on what a large number of points he had been wrong or displayed ignorance in the matter of Figs. 2 (a), (b), and (c), Plate 1 (facing p. 224 §). Starting with Bell's famous Technical Paper, the Author had intended to begin where Bell left off and to write his Paper for readers who were familiar with aprons, normal length and converging form of flanking guide-banks, and so forth. That had not been very successful and he had since found that some important links in the chain of design were missing. He would therefore like to introduce in Part II of his Paper the following amplification of the first sentence under the sub-heading "Location of Bridge Approaches" (p. 159 §).

The bridge tangent should be located at right angles to the general direction of the course of the river, and where possible so that the bridge will fall in the middle part of the tangent.

After the heading "Location of Bridge" (p. 159 §) he would like to interpolate a new heading, entitled "Bridge-Approaches", followed by the following remarks:—

It is a feature of the design of a guide-bank bridge that no spill-opening should be permitted in the approaches (1) between defined banks where these are to be found, or (2) where there are none, for such a distance from the bridge as will remove all reasonable possibility of the bridge being short-circuited by failure of the spill-opening.

In all cases a survey should be made of the country upstream of the bridge-approaches as far in each direction as the country is affected by flooding. The survey should show any defined channels, and such levels should be taken as are necessary to enable to be studied, in relation to the upstream length of the proposed guide-bank, (1) ponded afflux and (2) surge afflux against the banks and at the first flood opening on each side of the bridge, at maximum high flood. It is important that the effect of the afflux at the back of the guide-banks should be taken into consideration, as failures from this cause are not unknown.

No borrow-pits should be permitted on the downstream side of approach-banks or banks subject to afflux. This precaution is to lessen the danger of failure by seepage, and in addition to avoid the great danger of borrow-pits on the downstream side, near the bridge, becoming a channel for a part or the whole of the river.

The slopes at the back of guide-banks and the slopes of approach-banks subject to flooding should be protected by grass or quarry refuse or pitching stone as the circumstances require.

The method adopted by the Author was to take examples of guide-bank bridges with which he was familiar by plans and reports or by inspections, often after trouble with the guide-banks, and critically to examine their behaviour. The first example taken was the Curzon bridge over the Ganges at Allahabad (pp. 141 §-144 §). He had recalled during his visit in 1934 that after the guide-bank had been completed and whilst the

approach-bank was under construction, the water of the backwater channel under the left bank, during a minor rise in the river, had flooded the approach-bank borrow-pits and flowed out round the head of the guide-bank. To reduce the flooding he had dug across the sand bank a pilot cut-off channel which widened and deepened as the river rose further. He noticed that there was not enough room between the guide-bank and the left bank to admit the main river and that the length of the guide-bank, though barely sufficient to obviate undue obliquity of current at the piers, was more than sufficient to keep any embayment away from the approach bank. That led to consideration of the length of approach-bank which might be protected by the existing length of the guide-bank and that which might be protected by a guide-bank of greater length. The construction of Figs. 2 (b) and (c), Plate 1 (facing p. 224 §), had been fully explained on p. 143 §, and if the explanation were read in conjunction with perusal of the diagrams it would be found that none of the accusations was justified. The Author had not applied the term "natural radii of unrestricted curve in this part of the river" to the three radii, as alleged by the Director, but only to the 3,300-foot radius in Fig. 2 (b), and the assumption made in taking 1.75 as the cut-off ratio for the purpose of drawing the diagram was fully explained on p. 143 §. The Director presumably would have no objection up to that point, and the grounds of his opposition therefore must lie in Fig. 2 (c). It might be thought by an incautious reader of his remarks that the Director expected that the embayment shown in *Figs. 21 (a) and (b)* would curl round the end of the guide-bank towards the approach-bank, and that since it did not do so, no river would embay at the back of a guide-bank in any circumstances. Whether or not that was so, he did appear to imply that it would be impossible for a class A river to swing across from the axis of the bridge a distance of 3 miles to a permanent bank and then to embay at the back of a 2 *L* guide-bank. It had been mentioned in the Paper (p. 150 §), that the Kosi, a class A river, moved laterally from 12 to 14 miles in 7 years at a distance of 3 miles above the bridge, and such rivers had no difficulty in adjusting the gradient of their bed to changes in length. If objection were taken to the application of the same ratio of bend to chord in Figs. 2 (c), Plate 1 (facing p. 224 §), as in Figs. 2 (b), the Author was not very particular about it, as with a big bend such as that shown the river would be quite likely to cut off from higher up; it certainly would not tolerate such an increase in length. The only rivers which could be said to have a natural length were those not subject to bend and cut-off, in which the ratio would rarely exceed unity; the Gandak at Bagaha was a river of that nature. It could not be said that there was any "natural length" of rivers subject to bend and cut-off. The length of such rivers between fixed points varied from time to time, the variation depending on whether bends or cut-offs

were in the ascendant. A study of the length of the Ganges between Sarda and Pabna showed that since 1781, although the main stream differed in length, being sometimes longer and sometimes shorter in the years for which reliable information was available, the mean length had steadily diminished. Moreover, if, as seemed probable, the full development of the Damukdia cut-off led to the establishment of the direct channel between Mirganj and Raita as the main stream, the shortest length on record would be reached in the near future. It might be mentioned, as perhaps helping to make the matter clearer, that the Author recognized a difference in character between curvature to be found in the open *khadir* and that met with in connexion with heads of guide-banks or other similar natural features, by describing the former as "free" curvature and the latter as "constrained" curvature. There was very little relation between constrained curvature at the back of a guide-bank and "natural" meander length and amplitude, and, moreover, there was no such thing as a "dominant" discharge in alluvial rivers subject to large periodical variations of discharge, as was more fully described later. The Author believed that he had shown that he had not "made assumptions which were not justified", and that he "had taken into account" all relevant factors. He had also described how the idea arose from consideration of characteristics of the Curzon bridge and of the Ganges in the vicinity; and so far from assuming an embayment between the nose of the Curzon bridge guide-bank and the approach-bank, he had expressly given the reason why there was no such thing. It was difficult to believe that anyone would fail to see that the guide-banks in Figs. 2 (b) and (c), Plate 1 (facing p. 224 §) were diagrammatic guide-banks, and not the Curzon bridge guide-bank. Those diagrams would have served their purpose if they had established the general principle that the length of the approach-bank protected by a guide-bank was proportional to the length of the guide-bank. Although a general principle, which should always be borne in mind, it was not to be used unreasonably. The application of the general principle was governed by physical conditions and by railway engineering considerations. In some circumstances the length of the guide-bank would be governed by considerations of afflux. At the bridge over the Gandak at Bagaha there was an afflux of 6 feet at a point on the railway line 7 miles distant from the bridge, and that occurred with the line running upstream from the bridge but diverging from the river. From that circumstance certain deductions might be drawn.

There was no excuse for the Director's statement that the Author held that, where there was no permanent bank, there was almost no limit to the natural radius of curve or to the possible embayment; he had, indeed, taken pains to guard against any such misunderstanding by confining the demonstration between permanent banks, as at the Curzon bridge. Never-

theless he would give an example. The case he had particularly guarded against was where the river approached the back of the guide-bank from an unlimited distance on a course more or less parallel to the bridge tangent. The example would be obtained from the Hardinge bridge by assuming a western railway approach to be aligned on a prolongation of the bridge-centre line, and by drawing the line on *Fig. 13* (p. 187 §) of the Paper. In that case the channel known as the Jalangi channel, which was recently the main stream, would be found to have run, on a course more or less parallel to the bridge tangent, into the Sonaikundi bight or embayment, and it would be observed that that was an embayment at the back of an interrupted guide-bank of which Raita was the head. The river had come out of the Sonaikundi bight by a succession of cut-offs, and, as already mentioned, there was a probability that the Dadpur channel would shortly combine with the Damukdia cut-off. That example gave a very good idea of the scale of the Lower Ganges. It was put forward with the intention of showing that if a river were flowing more or less parallel with the approach-bank no addition to the length of the guide-bank could ensure the safety of the railway, but it showed instead that a natural feature, the Raita peninsula, did in fact serve that purpose. The Author, however, did not recommend artificial guide-banks of that length.

Figs. 21 (a) and (b) (pp. 282, 283, *ante*) furnished by the Director were of the greatest interest to the Author. From them it appeared that no dangerous eddies or other ill-effects were produced by the Sara protection-bank, which had been described as a protuberance and blamed for all the accidents at the right guide-bank. They also showed that with the river coming from Sara on the left bank at a discharge of 2 million cusecs, the pressure of that portion of the flood-discharge which flowed straight down the river was sufficient to keep the main stream, which was flowing out of the embayment along the Curzon head, in contact with the right guide-bank, and there was consequently no obliquity of current at the piers. Although he had previously described them as too short and of incorrect length, the Director now appeared to consider as the result of his model-experiment that those guide-banks were long enough, and the natural sequence would seem to be the withdrawal of his objection to the Sara protection-bank. The criterion for the sufficiency of the length of the right guide-bank was the degree of obliquity of current at the piers which might result from the interaction of the right guide-bank and the Sara protection-bank sited as shown in *Figs. 21 (b)*. In alluvial rivers with a discharge which varied from a maximum during the flood season to a minimum during the dry season, and of which the high-flood discharge varied from year to year, the maximum embayment was not to be found by running the high-flood discharge for 74 days. On the contrary, during a maximum flood, curvature tended to straighten out, cut-offs were initiated, and the river took as straight a

course as possible. It was during the fall in river-level that the main stream withdrew into the old channels and the banks began to be eroded. The rate of erosion might be greatest at three-quarter flood, but erosion continued until the river had fallen to a very low level, and the curvature continued to increase. The 2 million cusecs used by the Director in his model-experiment might be taken to be the maximum flood-discharge of the Ganges at Sara. It was a discharge which had not been exceeded or even approached for the last 30 years, during which high-flood levels had been taken or discharges recorded. The minimum dry-season discharge would be in the vicinity of 50,000 cusecs. If the year of maximum discharge were followed by years of a low high-flood discharge, such as occurred before the breach in the guide-bank, similar conformations in the river bed would follow. The sandbank immediately below the curve of the Sara protection-bank would begin to be formed, as might be seen in *Figs. 21 (a)* at the lower discharge of 1,700,000 cusecs. That sandbank would grow, until it stretched across the river, by continuous accretion during the annual fall of the river and year by year, until the constrained curvature of the embayment on the right bank reached a radius of, possibly, a little over a mile. By that time the main stream of the river, at three-quarter flood, flowing out of the embayment, would leave the Curzon head at a tangent and would pass through the left half of the bridge with great obliquity of current at the piers. Such obliquity would wreck the bridge by eddy scour at the piers involved. The right guide-bank was therefore quite definitely too short, not only for the spur condition at Sara but also for the curved-back Sara protection-bank. Sandbanks of the description referred to above, usually termed interior sandbanks, attained an extraordinary density and cohesion in the Lower Ganges. Offering considerable resistance to cut-offs, they played an important part in the formation of curvature.

With regard to the Gandak bridge at Bagaha, groyne had no place in the Bell-bund or guide-bank system, but so far as could be judged from *Fig. 3* (p. 145 §), the Gandak took no notice of the groyne. The river below the groyne picked up the curvature it would have taken without it, and proceeded as before. The Gandak at Bagaha, only 30 miles from the gorge, was a mountain torrent of great size. It ran, as might be expected, practically straight, with only such slight curvature as was incidental to its lateral movement. It passed over or around the groyne and continued its lateral movement as before.

After communicating his views on the Gandak bridge at Bagaha, the Director had taken up again the question of the origin of the breach in the right guide-bank, and, although that had been dealt with before, some reply to his remarks appeared to be necessary. It was regrettable that the Director did not follow the idea in the surge-wave illustration; the conditions leading to the surge-waves were described

on p. 222 §, lines 1 to 24. The information that the surge-waves lasted for 12 hours supplied the "something extraordinary" and the description mentioned above provided the "physical explanation", which might be considered to supply the convincing evidence which the Director alleged was lacking. The explanation that the surge-waves which were reproduced in the model were due to "river-breathing" was not convincing. What made the breathing was the question. Contrary to the Director's view, the Author believed that in the surge-waves, produced as he had described, he had found the long-sought-for origin of river-breathing. The Director had found some difficulty in understanding how sand could be washed out through the interstices of the pitching-stone covering by wave-action. The Author would content himself by saying that, whether the wave was passing along the face of a guide-bank or beating upon it, the damage was done by the down-draught of the receding wave, acting on the unstable sand-silt mixture which formed the core. The action of the small portion of the wave which entered the breach was thus described in his Paper by the late Mr. B. L. Harvey*: "This wave rushed across the embayment, dashed against the eroding bank, expended its force there, and then" at each recession "carried away large tracts of land, covered with jungle and trees, in strips 10 to 20 feet wide and 30 to 50 feet long." The reason why the wave, or rather succession of waves, had not produced surface wash-outs the whole way from the nose to the breach was that that part of the guide-bank had been protected during the three preceding years, 1931-1933, by means of masses of stone used in repairing so-called "slips", which were really "surface wash-outs" caused by surface disturbances. A succession, at 2-minute intervals, of such waves lasting 12 hours seemed to the Author to be something very extraordinary indeed.

The Director had made the point that a deep-seated slip, which extended by successive slips until the slip showed above water-level, gave the same indication of exposure of sand core as was given by failure of the soling and dropping of the stone pitching due to wave-action or to surface turbulence. That had certainly been the case, and reference to the exposure of the core as "a slip", in both of those very different cases, was the cause of all the confusion that had arisen. It was, therefore, proposed that such an occurrence should be referred to as a "core-exposure" until its character had been ascertained, when it could be described either as "a deep-seated slip" or "a surface wash-out", as the case might be. A deep-seated slip might or might not be a very serious matter, but a surface wash-out, if attended to at once, would easily be restored. In the case of the breach in the right guide-bank there were three adverse circumstances: (1) the core exposure occurred after midnight; (2) the surge-wave action which lasted for 12 hours was of unusual

§ *Ibid.*

* "The Restoration of the Breach in the Right Guide Bank of the Hardinge Bridge." *Journal Inst. C.E.*, vol. 4 (1936-37), p. 21. (November 1936.)

intensity, so that the breach was complete before the damage was discovered; and (3) the accident happened in the early part of the Pujah holidays, so that it was only with the greatest difficulty that labour was procurable for repairs. The Author claimed that it had been established that the breach arose from a core-exposure of the surface wash-out variety due to surge-waves set up immediately above the guide-bank head, that it was owing to the unusual character and intensity of the waves that the core-exposure was converted into a breach before it was discovered, and that its occurrence at an early hour and during the Pujah holidays had turned a simple core-exposure into a disaster.

As the Director had made no reply to the Author's analysis of the model-experiment, which was alleged to show that the breach was due to a deep-seated slip, it was evidently unanswerable. The analysis would be found in the Author's reply to the Discussion on the Paper, p. 223 §. Instead the Director had said that he had reproduced the slips on a geometrically-similar model, but as he had shown no signs of amending the planning of his experiment—except with regard to the vertical scale—two of the three insuperable objections taken by the Author to the previous experiment would still apply.

To continue the story of the circumstances which turned into a disaster a simple surface wash-out, which if discovered in time could ordinarily have been repaired in a few hours, it had to be related that the repair of the breach had taken 2 years and had cost £750,000. That, however, was not the end of it. The Committee of Engineers, appointed to prepare a scheme to supplement the existing training and protection works, invited the Hydrodynamic Research Station at Poona to carry out some model-experiments for them. That resulted in a recommendation to dismantle and abandon the Sara protection-bank which, without further consideration, was carried out. As Sara was the key position or pivotal point which, for 150 years, had kept the Ganges from wandering at large through the Pabna district, that action was nothing short of sabotage. The result would be seen in the necessity to construct another Sara protection-bank as near the old one as practicable, and, in addition, a dam with sluices across the Ganges spill. Those works would cost not less, and probably much more, than £250,000, thus bringing the cost of failure to detect, in time, a small core-exposure in the right guide-bank up to a matter of £1,000,000.

The Director had found it difficult to reconcile the Author's statements about erosion at Raita and the left bank at Sara. The statement about the head of the Raita peninsula seemed to be perfectly clear. There was very little of the resistant "Sara" clay left at Raita. There was a west face and a north face, and the Author had joined them together by the easiest curve practicable and had revetted the resultant Raita protection-

bank to the best of his ability for a scour of 100 feet below low-water level, and although the scour had lately proved to be double that depth there had never been any serious difficulty in maintaining it, and it was standing now better than ever (p. 172 §, lines 10-14). With regard to Sara, the whole of the $\frac{1}{2}$ mile of erosion had taken place between 1780 and 1868, and there was no record and no tradition of any erosion at Sara after the latter date. By the time that the bridge was opened to traffic the railway ferry had been worked from the same spot at Sara for 47 years and the river had oscillated within narrow limits in passing across the head of the Raita peninsula. The head of the Raita peninsula had been eroded $2\frac{1}{2}$ miles since 1780, and it appeared to be doubtful whether the river would ever again attain the extreme embayment of the Lalpur bight. Moreover, the river was believed to be increasing in size, owing to the silting up of the Nadia rivers, and it was known to be straightening its course; it therefore appeared to be safe and reasonable to await events instead of incurring a large expenditure on works which might never be required. What the Author had not foreseen was that, of the rapid succession of chief engineers who were responsible for the maintenance of the bridge, or among their staff in more immediate charge during the vital period of 10 years, there would be no one who would realize the possible consequences of the erosion of the bank above the Sara protection-bank or that there was any need to seek advice. It had been customary, at the request of the local staff, for the Government of India to consult the builder, if he had retired, or any of the large bridges, before carrying out any important modification or repairs, and the omission in the present case was most unfortunate. If the Author had received earlier information of what was taking place, it would have been possible to lengthen the right guide-bank, or at least to reduce the length of the gap between it and the Damukdia extension. To return to the Director's argument, the length of the bank revetted at Sara was 3,650 feet, and the Director's suggestion of a Curzon head at the upstream end of the Sara guide-bank and a similar extension of the head of the right guide-bank was not a satisfactory solution. The Author had already shown that extreme obliquity of current at the piers would ensue, and the Director would no doubt have observed that the direction of flow off a Curzon head from a deep embayment would not be, by any means, so favourable as that from the curved-back protection-bank delineated in *Figs. 21 (a) and (b)* (pp. 282, 283, *ante*). The solution proposed by the Director came back to the Author's argument for the short guide-bank of 30 years ago that, if the river were directed down towards the bridge from Sara, it had to go straight through it. The fallacy seemed to lie in the accretion of the sandbank below Sara during years of low discharge. In any case, since it was now known that the scour at the head of a guide-bank in Ganges sand-silt was more likely to be 200 feet below low-water level

than the 100 feet anticipated 30 years ago, it was obligatory to remove the heads of guide-banks as far away from the bridge as was practicable.

There were a few expressions frequently used by the Director in argument to which the Author had been unable to attach any meaning, such as "... a guide-bank tended to pull a river around its upstream nose" and the Damukdia guide-bank, by "pulling the stream round its tail ..." made the destruction of the right guide-bank inevitable." The former had been traced to its origin in the Director's Final Note for the Hardinge Bridge Committee dated 1st November, 1935, where it appeared as one of the results of model-experiments, thus :

"(i) Water tends to adhere to guide-banks and leaves them with reluctance.

"(ii) A guide-bank pulls the river round its upstream nose, but there is a limit to the embayment caused thereby." Below those results on the same page he had said :

"I would also call attention to Sir Robert Gales's remarks on the Sara guide-bank.

"... the bank at Sara ... was revetted with pitching stone for a length of 3,650 feet.

"As soon as *this was done*, the bank *immediately upstream* of the revetment began to be eroded ..."

"The portion in italics explains the whole trouble about guide-banks. No matter what you do erosion upstream of a guide-bank must occur and should be allowed for ; but this erosion is definitely limited."

"There is little doubt in my mind that the Sara revetment was the direct cause of the trouble and there is no doubt the Damukdia guide-bank intensified it."

The Author took the strongest exception to the use of his name in connexion with such an absurd deduction. The statement had not been made by him ; if it had been made by any other person it was false ; if it had been true it would have been merely a coincidence. Nevertheless, out of that fog of unreason the Director had been able to spin the cryptic slogan about the "upstream" nose which was to lead to the dismantling of the Sara protection-bank, and so to a vast additional expenditure in restoring the Sara defence work in a retired position, and in retiring the main line and building a dam with sluices across the Ganges spill. The revetment at Sara was completed by 1914-15. In the diary of events (Appendix I of the Report of the Hardinge Bridge Committee) the first mention of erosion above the head was in 1925-26, 11 years after the revetting was completed. Instead of "a guide-bank pulls the river round its upstream nose", all that could be said on the evidence was that an unprotected erodible bank eroded more rapidly than a protected erodible bank. There was, however, evidence, since disclosed, that the 3,650 feet of bank revetted at Sara comprised the last remnant of the resistant Sara clay. That was visible as a shelf just above low-water level and projecting beyond the line of the erodible bank. The inference was plain that the erosion of the bank upstream of the Sara clay remnant would have proceeded more

rapidly than the erosion of the remnant, and that the conditions stated by the Director to explain "the whole trouble about guide-banks" would have occurred at the natural river bank at Sara.

In his contribution to the Correspondence on the late Mr. Harvey's Paper,* the Director had described a very interesting model-experiment in the following words:

"When Mr. Inglis had first visited the bridge . . ." there was a " . . . large-radius, slow-moving eddy, which revolved slowly in the vicinity of the breach . . . ; the true explanation—as viewed through a glass sheet let into the side-slope of a model of the right guide-bank—was that below the slowly-revolving surface eddy there was a region of instability at mid-depth, where the flow was sometimes in one direction and sometimes in another; but the only severe action was at the bed, at the toe of the pitching, and there the flow was straight and turbulent, coming in surges, some stronger than others, at the rate of from one to three per second in the model. Sometimes sand was picked up in little gusts like dust on a road. Stones were carried away in the surges, and although a stone might remain unmoved for several seconds, it might then be carried away in a powerful surge."

As the above followed a statement that "a model designed to determine the exact way in which the breach occurred had reproduced the breach accurately", it had to be assumed that the inference drawn by the Director was that the breach had been caused by the severe action at the bed which picked up sand and carried away pitching stones. The argument was not very close, as the experiment was carried out under conditions, such as depth of scour and existence of a slow-moving eddy, which were the result of the breach, and not the cause of it. However, such as they were, those were the views of the Director in 1937. Great, therefore, was the Author's astonishment to find in the communication under review that, after mention of an eddy at C₂, Fig. 2 (b), Plate 1 (facing p. 224 §), and the whirlpool at Sara, the Director continued:

"Model-experiments showed that such eddies, which generally were found near places where slips occurred, were merely superficial, extending down to about half depth only, and that they resulted from surface water, flowing away from a guide-bank at an angle, 'brushing' past the relatively stationary surface water near the guide-bank, causing it to revolve with a low velocity. So far from such eddies and surge-waves being the cause of damage, they were both secondary effects, the latter being due to what Mr. Inglis described as severe action at the bed, at the toe of the pitching, where the flow was straight and turbulent, coming in surges, some stronger than others. Stones were carried away in the surges, and although a stone might remain unmoved for several seconds, it might then be carried away in a powerful surge."

It was difficult to reconcile the views of 1937 with the views of 1939. There was evidently no need for the Author to attempt to controvert the views of 1937, as the views of 1939, that eddies and surge-waves, being secondary effects, could never be the cause of damage, conclusively disposed of them.

* Correspondence on "The Restoration of the Breach in the Right Guide Bank of the Hardinge Bridge." Journal Inst. C.E., vol. 6 (1936-37), p. 330. (October 1937).
§ *Ibid.*

As no further reliance could be placed on the planning or interpretation of the Poona model-experiments, the Author ventured to put forward some of his own views. Stationary river eddies, although secondaries, were not necessarily inert. The capacity of eddies for damage depended on their size and intensity and on their position in relation to works. It would be sufficient to consider only large eddies of considerable intensity and to recognize two classes as (1) "confined" and (2) "free." A confined eddy was one confined in contact with a work, such as a guide-bank, by outside forces, and such eddies were very destructive. A free eddy was one which would be free to move away from immediate contact with a work such as a guide-bank which would otherwise hinder its development.

The eddy at Sara, which had eventually become a whirlpool, might be cited as an example of a confined eddy. The eddy had been caused by the main stream, or, in the earlier stages, by the fringe of the main stream, issuing from an embayment above the upstream end of the original Sara protection-bank and thereby crossing the end in such a manner as to cause the formation of an eddy, in the acute angle between the centre-line of the protection-bank and the direction taken by the current. That eddy, which formed over the shelf of resistant Sara clay, was naturally not very deep, but it soon showed its character by cutting through and destroying the upstream end of the protection-bank, of which it left remaining a spur at ground-level and beyond that the sunken spur, which was the original end of the protection-bank. The eddy was at first held in position and pressed into the protection-bank by the part of the main stream which was trying to run straight down its old channel, but the whole main stream was engaged in cutting its future channel in the direction of the upper part of the Damukdia guide-bank. To that it was urged by the increase in the depth of embayment, by the deflexion caused by the sunken spur, and by the thrust of the eddy, which, with its back to the wall or protection-bank and increasing in size, was aiding the lateral movement. Later, as soon as the lateral movement of the main channel had released the eddy, it moved further out, and by the time its diameter had increased to 800 feet and the depression at the centre had become noticeable there was no reason to doubt that it was extending downwards towards the river-bed. By that time it had become a free eddy and as such could do no damage.

The development of a free eddy, having its origin at a Curzon head, might be described in the following way. As soon as the main stream attempted to leave the head on a tangent, there was a reduction in pressure and a lowering of the surface-level between the guide-bank and the main stream, which induced a return current running upstream along the guide-bank. That current, at its origin a purely surface current, restored the pressure and released to some extent the tension or tendency to cling to the guide-bank, and allowed the main stream to run a little further out. The

water of the return current, being caught and carried down by the main stream, was the immediate origin of the eddy, which released the surface water of the main stream to take its tangential course. The eddy energized by the main stream increased in diameter and depth, and, there being nothing to prevent it, moved downstream and took up a position suitable to its increasing size between the guide-bank and the main stream. The axis of a stationary eddy being necessarily vertical, it would appear that, as an eddy extended downwards over a fully developed apron, the lower part of the eddy would be interfered with by the stone-protected slope, and the disturbed condition and diving flow of the model-experiment would be introduced; alternatively, the eddy might move down to a position where, whether by eddy-action or diving flow, it would not damage the guide-bank. It was to the freedom of movement of that eddy in ordinary cases that the Author ascribed the absence of damage to guide-banks by eddies in the position under consideration. That the eddy in the model-experiment remained stationary was due in the Author's opinion to the circumstance that the pull of the main stream back towards span No. 2 confined the eddy in the position it occupied. It would seem from the model-experiment that the slowly-revolving eddy was the cause of the disturbed conditions and consequent scour, and that the surface appearance of the eddy sufficiently marked the position of the scour.

It had been stated by Colonel Macrae that he had been unable to find any record of attack on, or of eddies at, the tails of guide-banks below a bridge, and as that was also the Author's experience he suggested that, such eddies being free, they were for the most part harmless. Another example of a free eddy was the large eddy which formed at the tail of the Damukdia guide-bank and took up a position where it was quite harmless to the guide-bank which caused it. It was that harmlessness of the free eddy which made possible what was known as the "interrupted" guide-bank. The large slowly-revolving eddy in that case was at a distance of not less than 3,500 feet from the head of the right guide-bank, and, being necessarily out of the line of flow of the main stream, it could have had no ill-effect upon the guide-bank. It was the Author's considered opinion that the Ganges could not be bridged at any point between Sarda and Pabna, at any reasonable cost, unless the general principle of the interrupted guide-bank was accepted as sound practice. It had indeed already been accepted by the inclusion of the Raita protection-bank connected with the bridge and adjacent works by banks with a sufficient margin above maximum high-flood level. The Author's proposals for the left bank originally allowed a gap between the Sara protection-bank and a lengthened left guide-bank, a gap which had since been omitted in deference to the prevailing view. Owing to the delay in bringing the matter to notice, the gap between the right and Damukdia guide-banks was far too wide for the splayed alignment, and the design of the tail of the latter guide-bank left much to be desired. Although the Damukdia extension had

served its purpose in saving the bridge, the gap was too near the bridge and it should certainly be closed at the earliest opportunity.

The Author regretted that the Director should have been misled by the term "clay-patch strata", which had received the name in the same way as the Indians of North America. The brown clay patch first met with soon washed away, leaving under the right guide-bank near the abutment a thin band of blue clay, below which strata of coarse sand, partially indurated, extended under the whole of the upstream part of the guide-bank. Those strata resisted erosion, and they probably stood with a vertical face until they gave way suddenly to cause a slip, as at chainage 5.5 to 10.5. That had already been described more than once, and the different depths at which the strata were met with at the abutment and at the nose of the guide-bank had been given in explanation of the difference in character of the slips at those places. The so-called clay-patch strata differed in every way from the formation which caused the difficulties in maintenance on the curved-back part of the Sara protection-bank. The difficulties did not arise from the true Sara clay or its counterfeit, but from the existence at Sara of beds of fine Ganges sand with an unusually large admixture of silt, and the difficulty was not, as with the clay-patch strata, to get them to slip, but to get the sand-silt beds to stand at the weak spot on the curved-back part of the protection-bank. That could have been done, as previously explained, by using the pitching stone stacked at the top of the bank for that purpose. All that was necessary was to have laid down an apron by dropping the stone through water and then to have made up the slope with stone, for the short length of the slip. The stone recovered by dismantling the Sara protection-bank had chiefly come from that reserve, which should have been used as described. The quantity of stone recoverable from the permanent slope of a fully-developed protection-bank was negligible (see Figs. 10 (b) and (c), Plate 1 (facing p. 224 §)). Some remarks on the design of guide-banks with bands of clay below apron-level would be found on p. 172 §.

The Author would be gratified to receive a copy of the Technical Paper on Aprons under preparation at the Poona model-experiment station. Meanwhile, what was required was a design for the apron and protective covering of the head of a guide-bank for the Hardinge bridge in the local sand, and the present was the sixth year of the model-experiments. The Author hoped that the Director would be satisfied with the list of advantages of the convergence upstream of the flanking guide-banks of a railway bridge supplied above with the revised design of guide-banks in the Author's reply to Colonel Macrae's communication. The Author's views on model-experiments were that there was a great future for them, when, with good planning and correct interpretation, great advances would be made.

The principle of the permeable dyke was probably invented in India. In India to-day *bandals* were used to silt up one, and open out the other, of alternative cold-weather channels of the Ganges. *Bandals* were rows of bamboos worked into the sand and connected and steadied by string. Permeable dykes of long Oregon piles connected in groups of three piles had been tried with varying success. The permanent permeable dykes used in Argentina were an advance on Indian practice, but before it could be said whether they would be successful in India further information would be necessary as to the seasonal variation, and maximum discharge, of the rivers in which they had been used with success in Argentina.

The Author regretted that he was unable to give more time to the remaining communications owing to the War, but he believed that Mr. Trench would find that most of his requests for information had already been complied with.

Paper No. 5143.

"The Total-Heat-Entropy Diagram for Diphenyl."†

By SIDNEY JOHN ELLIS, Assoc. M. Inst. C.E.

Correspondence.

Mr. W. L. Badger, of Michigan, observed that three substances should really be considered: diphenyl, diphenyl oxide, and Dowtherm A. Diphenyl had a boiling point of 490°F. and a freezing point of 154°F. ; diphenyl oxide had a boiling point of 498°F. and a freezing point of 81°F. ; Dowtherm A was an eutectic mixture of the two, having a freezing point of 54°F. , and it contained 73.5 per cent. diphenyl oxide and 26.5 per cent. diphenyl. The thermal properties and stability of diphenyl and diphenyl oxide were so nearly alike that thermodynamic data derived for either might be used for the other, or for the eutectic mixture, with an accuracy probably within the limits of precision with which any basic data then available were known.

The thermal properties of those compounds had been discussed in more or less detail by a number of different authors. One of the early advocates of the binary cycle for power generation was Dr. H. H. Dow*, who presented considerable thermodynamic data for diphenyl. Two outstanding Papers giving the properties of Dowtherm A were those of

† Journal Inst. C.E., vol. 10 (1938-39), p. 227 (December 1938).

* "Diphenyl Oxide Bi-Fluid Power Plants." Journal Am. Soc. Mech. E., vol. 48 (1926), p. 815 (August 1936).

Gaffert ‡ and of Ullock, Gaffert, Konz, and Brown||, whilst data on diphenyl had been recalculated by Findlay††.

The Mollier charts given by the Author and by Gaffert checked very closely, but the specific heats of the liquid presented by them did not agree at all with those of Ullock, Gaffert, Konz, and Brown. An examination of all of that data had been made in considerable detail but the source of the difference was not yet apparent. The Author's curve for a pressure of 5 lb. per square inch absolute was not in agreement with the data given in his Tables, nor with the values in Gaffert's diagram. The values given by the Author in his Tables for that pressure did, however, check Gaffert's values.

The Author had presented several representative cycles for binary systems, and his efficiencies compared very closely with those given by Dr. H. H. Dow in the early publication, whilst Gaffert had discussed the matter in greater detail than either. Probably the greatest difference of opinion of those three authorities lay in their choice of pressure for their high-temperature turbine. The Author advocated expanding to a pressure of 1 lb. per square inch absolute; Dow suggested expansion to atmospheric pressure, or to a moderately high vacuum; Gaffert, on the other hand, had gone to considerable trouble to show that the most economical exhaust pressure for diphenyl oxide, or for similar compounds, was approximately 25 lbs. per square inch absolute. Gaffert showed the possibility of high economy from diphenyl-oxide turbines, and its advantage due to the large available supply. Dow, on the other hand, pointed out the limitations of excessive high-pressure steam due to the decrease in accessory efficiency; he stated that it was mathematically possible to demonstrate that an overall efficiency of only about 28 per cent. should be expected from steam systems, whereas those calculated for binary systems by the Author and Dow were 33 and 34 per cent.

Mr. Badger thought that it was unfortunate that the Author had apparently not taken into consideration (or at least referred to) the previous literature on the subject.

Mr. J. F. Field felt the Author was perhaps too optimistic regarding the practical possibilities of a binary-fluid arrangement using diphenyl and steam. He described a cycle in which heat was taken in by diphenyl at a pressure of 200 lb. per square inch absolute, corresponding to an upper heat-cycle temperature of 784.3° F. The efficiency of the cycle worked out at 49.6 per cent. (Example 1) and the Author claimed near the bottom of p. 235 §, that such an arrangement might have an actual efficiency of

‡ "High-Pressure-Steam and Binary Cycles as a means of Improving Power-Station Efficiency." Trans. Am. Soc. Mech. E., vol. 56 (1934), p. 755.

|| "Thermal Properties of Dowtherm A." Trans. Am. Inst. Chem. E., vol. xxxii (1936), p. 73.

†† "Some Suggestions for Diphenyl Heat-Engine Cycles." *The Power Engineer*, vol. xxix (1934), p. 89 (March 1934).

§ Page numbers so marked refer to the Paper. (Journal Inst. C.E., vol. 10 (1938-39), p. 227 (December 1938).—SEC. INST. C.E.

33.58 per cent. The ratio of those two efficiencies was 67.7 per cent. Reference to Mr. Field's Paper * indicated that there was no authentic record in Great Britain of a steam plant reaching a higher ratio than about 62.5 per cent. of the ideal steam-cycle efficiency, in spite of the intensive development of steam plant over many years. In a binary-fluid arrangement the inevitable temperature-drop in transferring the heat from the upper- to the lower-temperature fluid seemed to indicate the prospect of a somewhat smaller ratio between the efficiency of the engine and the ideal efficiency of the heat cycle.

The Author did not mention the specific volume of diphenyl gas, but since the ratio of weight of diphenyl to weight of steam in the cycle described was of the order of 6 to 1, the physical properties of the fluid might entail difficulties in mechanical construction of a suitable turbine.

The Author compared the diphenyl-steam engine with a mercury-vapour-steam arrangement which had been tried in America, and from which it was hoped to achieve a thermal efficiency of 35.9 per cent. There was no evidence yet that such a figure had been reached for any length of time, although experiments had been proceeding for a number of years. There was little doubt, however, that such efficiencies were possible with mercury if the various physical difficulties already met with could be overcome.

It was instructive to study the essentials of the mercury cycle from which that performance was anticipated. Mr. Field believed that the mercury took in heat at a temperature of about 884° F., corresponding to a pressure of 70 lb. per square inch gauge, and that the binary cycle had an ideal efficiency of about 57 per cent. The expected ratio of efficiency was therefore about 63 per cent. and that might be optimistic in view of the temperature-drop required between the two working fluids. There was little doubt that a diphenyl engine, taking in heat at 784° F., would be at some disadvantage with such a mercury arrangement, taking in heat about 100° F. higher; further, with mercury still higher temperatures could be arranged with comparatively easy pressure conditions.

With steam it was now technically feasible to operate at, say, 2,500 lb. per square inch gauge and 1,000° F. total temperature, with reheating at, say, 600 lb. per square inch gauge to 825° F. and with feed-heating to 550° F., which would give an ideal-cycle efficiency of 54 per cent. An efficiency-ratio of 62.5 per cent. could reasonably be expected with those conditions, giving a working efficiency of about 33.5 or 34 per cent.

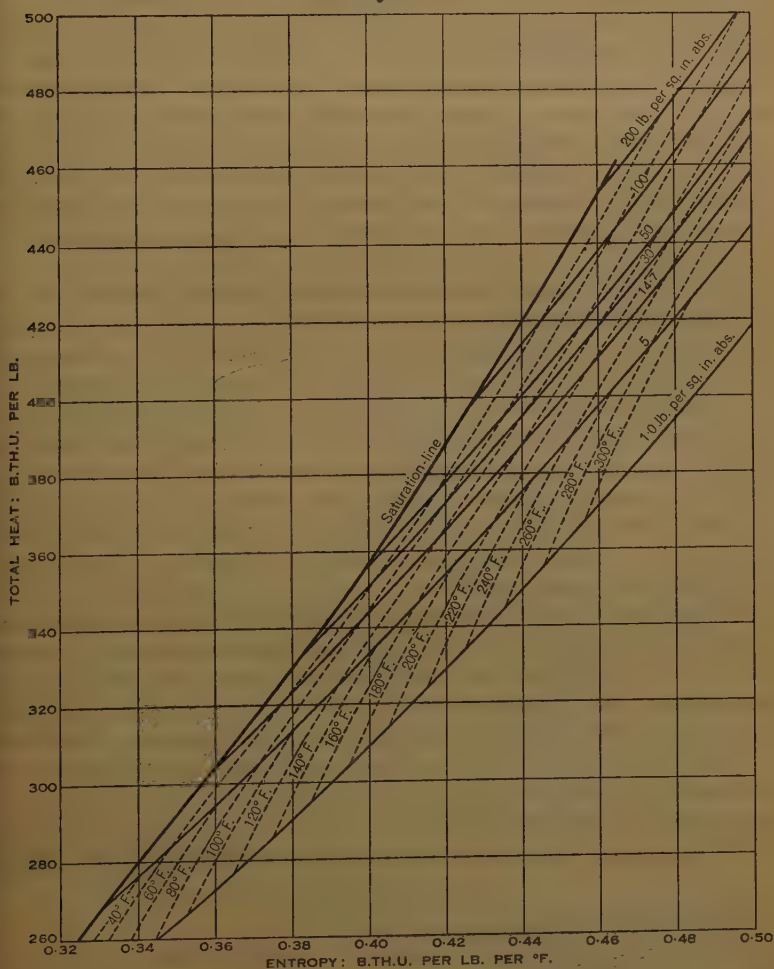
In those circumstances it seemed difficult to justify the use at the present time of any fluid other than steam, which had physical properties of unique convenience which were not always appreciated. Efficiency depended mostly on the upper limit of temperature, irrespective of pressure, and

* "A Suggested Basis of Comparison for the Efficiency of Steam Turbo-Generator and of Steam-Electric Generating Stations." *Journal Inst. C.E.*, vol. 10 (1938-39), p. 241 (December 1938).

Mr. Field felt that if the metallurgical problem were solved for any relatively low pressure, suitable mechanical designs for the higher pressures required with steam would follow.

Mr. J. R. Finniecome observed that diphenyl and diphenyl oxide, and the mixture of both, known as "Dowtherm", were known principally as heat-transfer media at temperatures of from 500° to 700° F. and at corre-

Fig. 4.



spondingly low pressures of from 15 lb. per square inch absolute to 115 lb. per square inch absolute for reheating steam and for preheating air.

The use of skew co-ordinates for the total-heat-entropy diagram was more suitable for indicating, graphically, the complete cycle of diphenyl for

the liquid, the saturated and the superheated regions. Mr. Finnicome found that the rectangular co-ordinates generally used for total-heat-entropy charts for steam, air, and other gases, had the advantage that the heat-drop could be scaled directly off the vertical line for any specified conditions. He had prepared such a chart (*Fig. 4*) for the superheated region based on the Author's data contained in the Tables. Incidentally, he wished to point out that there was a misprint in Table V (p. 237 §) : for a temperature of 60° F. the entropy should be 0.3319, not 0.3219.

Mr. Keith Fraser observed that an interesting allied use of diphenyl occurred in the Bremono Bluff power-station of the Virginia Public Service Company, in the United States, where a "Dowtherm" economizer was located directly above a single-pass boiler with a heating surface of 19,830 square feet, which was extremely high, making the gas outlet and source of heat for preheating air a considerable distance from the pulverizers on the ground floor. The heat was then transmitted through the "Dowtherm" system to the air-heaters to pulverizers at ground-floor level.

The particular reason for using the eutectic mixture referred to in the Paper was that the vapour was also in the same proportions of diphenyl and diphenyl oxide as the liquid, and the condensate was therefore in the same proportions, thus maintaining equilibrium in the system. In addition to that, the eutectic had a lower freezing point (52.7° F.) than pure diphenyl, which froze at 154° F. The eutectic would therefore appear to be a more suitable medium than pure diphenyl as described by the Author. It was of interest to note that further methods of freezing-point depression had been used by Lucas* and others. The physical properties of the eutectic did not differ, so far as was known, very greatly from those given by the Author.

Dr. H. H. Dow advocated the use of diphenyl oxide as a rival to mercury and binary-fluid plants†. It did not decompose at the temperatures required in the boiler, and its vapour was about 9.4 times the weight of steam vapour, with the result that a turbine with that substance would run at lower speed with higher torque at the same horsepower. Mr. Fraser might draw attention to Dr. Dow's patent||, while interesting data on heat-transfer coefficients†† and liquid-film heat-transfer coefficients‡ had been given by Mr. W. L. Badger.

The Author, in reply, observed that he was indebted to Mr. Badger

§ *Ibid.*

* British Patent Nos. 398,492 and 427,170.

† "Diphenyl Oxide Bi-Fluid Power Plants." *Journal Am. Soc. Mech. E.*, vol. 48 (1926), p. 815 (August 1926).

|| U.S. Patent No. 188-311.

†† W. L. Badger, "Heat Transfer Coefficients for Condensing Dowtherm Films." *Industrial and Engineering Chemistry*, vol. 29 (1937), p. 910 (August 1937).

‡ D. S. Ullock and W. L. Badger, "Liquid-Film Heat Transfer Coefficients." *Industrial and Engineering Chemistry*, vol. 29 (1937), p. 905 (August 1937).

for the references to previous Papers. The Author's Paper had been completed in midsummer 1936, and although the usual search had been made he had failed to find any reference to the excellent Paper by Gaffert (to which the Author had now had limited access), or to the later one by Ullock, Gaffert, Konz, and Brown.

The specific heats of liquid diphenyl given in the latter Paper had been checked with those in the Author's Paper, and, contrary to the opinion expressed by Mr. Badger, they appeared to agree very well. Perhaps Mr. Badger was considering the vapour, rather than the liquid. On the other hand, the specific heats of the superheated vapour of diphenyl taken from Mr. W. S. Findlay's curves did not agree with the specific heats of the vapours specified in Gaffert's Paper, and differed considerably from the values given in the Paper by Ullock, Gaffert, Konz, and Brown, which themselves differed from Gaffert's earlier values. That difference appeared to be a matter for further investigation. The Author had checked the curve for a pressure of 5 lb. per square inch absolute, and it appeared to be consistent with the tabulated data.

The comments of Mr. Field on the more practical possibilities were very interesting. He apparently considered an efficiency ratio of 67 per cent. too high by comparison with the figures tabulated in his own Paper. It would appear, however, that the efficiencies stated in Mr. Field's Paper referred to overall station efficiencies averaged over a period of 12 months, and covering losses not normally incurred under simple test conditions. Mr. Field remarked that efficiency depended mostly on the upper limit of temperature irrespective of pressure. That, of course, was substantially true, but the modern steam cycle obtained its high efficiency by a combination of high temperature and high pressure, as could be seen from the last Table in Mr. Field's Paper *.

The practical interest in binary-fluid systems lay in the possibility of obtaining the advantages due to high temperature without the disadvantage of high pressure, thereby making it possible to utilize cheaper materials of construction, with a consequent great saving in capital cost.

The Author was obliged to Mr. Finnicome for pointing out the error in Table V (p. 237 §), and for the chart for the superheated region. He was also indebted to Mr. Fraser for his remarks on the "Dowtherm" economiser, and for the references, which would be of value to other workers in the field.

* Footnote (*), p. 334.

§ *Ibid.*

Paper No. 5186.

"A Suggested Basis of Comparison for the Efficiency of Steam Turbo-Generators and of Steam-Electric Generating Stations."†

By JAMES FREDERICK FIELD, B.Sc., Assoc. M. Inst. C.E.

Correspondence.

Mr. W. T. Bottomley observed that the modified ideal Rankine cycle which the Author suggested as a criterion for the performance of turbo-generator plant when feed-heating and reheating were adopted was not new. In 1924 Mr. Bottomley had used that form of modified cycle, with the same form of explanatory diagram as *Figs. 6* (p. 248 §), to show the theoretical thermodynamic advantage of high-pressure reheating and feed-heating at North Tees power-station *; that was the first station in which those cycle improvements had been put into practice, and it was the prototype of several similar stations in the United States. It had also been pointed out * that the theoretical thermodynamic advantage due to reheating only accounted for a portion of the practical gain, and that the greater part of the gain was due to the improvement in the turbine efficiency due to the reduction in the wetness-loss at the low-pressure end of the turbine. In addition to that, the amount of reheating per lb. of initial steam was in practice considerably less than the amount of reheating done in the ideal cycle, partly because a portion of the ideal reheating was done by the turbine-losses carried over from the high-pressure cylinder, and partly because the steam extracted for the high-pressure feed-heater did not pass through the reheater. At Dunston power-station the actual reheating per lb. of initial steam was only 75 per cent. of the reheating done in the theoretical cycle.

Whilst, therefore, the modified Rankine cycle was useful for explaining the theoretical advantage of reheating, it was to a certain extent misleading as a basis for comparing the actual advantage. An examination of the Table at the end of the Paper (p. 251 §) would show the extent of the error. Comparing Dunston "B" and Barking "B" stations, the steaming conditions were the same except that there was reheating at

† Journal Inst. C.E., vol. 10 (1938-39), p. 241 (December 1938).

§ Page numbers so marked refer to the Paper (Footnote (†) above).—SEC. INST. C.E.

* See Figs. 10 and 11 on pp. 57 and 58 of second part of article entitled "The North Tees Power Station." *Engineering*, vol. cxvii (1924), p. 753, and vol. cxviii (1924), p. 57.

Dunston. The reduction in heat-consumption due to reheating was, according to the figures given by the Author for the ideal-cycle efficiencies, only 1.6 per cent., but in practice it should be at least 5 per cent. The error therefore due to judging the effect of reheating by means of the ideal cycle suggested by the Author was of the order of $3\frac{1}{2}$ per cent. The same discrepancy, but to a lesser extent, occurred due to the wetness-loss when comparing the effect of increasing the initial steam-temperature for straight cycles without reheating. Comparing the ideal cycles for Battersea L.P. and Barking "B" stations, the steaming conditions were the same except that the initial temperature at Battersea was 50° F. higher. According to the ideal efficiencies the reduction in heat-consumption at Battersea was 0.7 per cent., but actually the reduction should be about 1.8 to 2.0 per cent., giving a discrepancy of over 1 per cent. for 50° F. If the temperature at Battersea had been 950° F. instead of 850° F., without raising the pressure, the discrepancy would have been over 3 per cent.

On the other hand, when considering the effect of increasing the initial pressure without increasing the initial temperature and without reheating, the discrepancy was the other way: the increase in initial pressure would increase the wetness-loss so that the improvement shown by the ideal cycle should always be greater than the actual improvement.

It would be noticed that in the stations shown in the Author's Table a rise of pressure was accompanied either by a rise of temperature or by reheating, so that the discrepancies indicated above tended to cancel out. In those examples a comparison on the basis of the ideal cycle did tend to show the relative operating efficiency. Dunston "B" and Deptford West stations were, however, exceptions. There were other stations where the temperature was 700° F. but the pressure ranged from 250 lbs. to 600 lbs. per square inch, and in those cases a comparison on the basis of the ideal cycle would be misleading.

It would be admitted that the exceptions in the Table were not apparent from the actual performances shown, but the figure for Deptford West station was given for 1930, which was before the era of the modern efficient boilers with water-cooled furnace-walls, whilst the operating figure for Dunston "B" station was given for the year 1936 when the loading and operating conditions were not as good as they were at present.

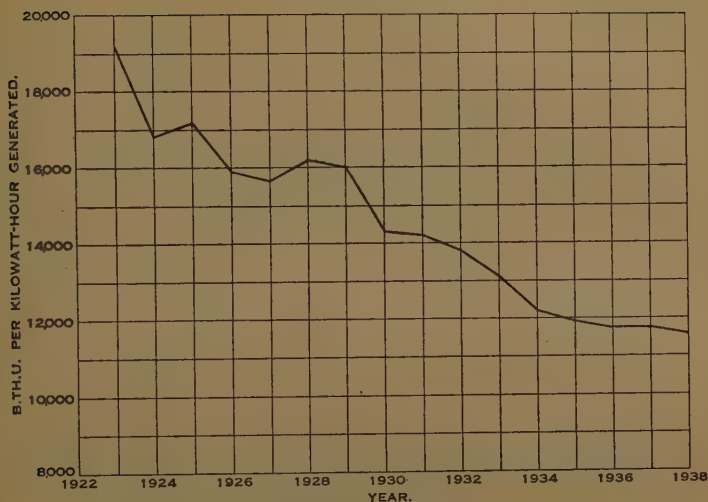
Mr. J. R. Finnicome observed that a standard of comparison of the ideal efficiency of an engine had first been considered by Mr. P. W. Willans in 1888*. Prior to that there existed the ideal Carnot cycle, known now for nearly a century, and the Rankine cycle, developed in 1854. Both were in general use, but Rankine's cycle was considered specially suitable for the steam cycle, as it approached more closely the actual

* "Economy Trials of a Non-Condensing Steam-Engine: Simple, Compound and Triple." Minutes of Proceedings, Inst. C.E., vol. xciii (1887-88, Part III), p. 128, and vol. xevi (1888-89, Part II), p. 230.

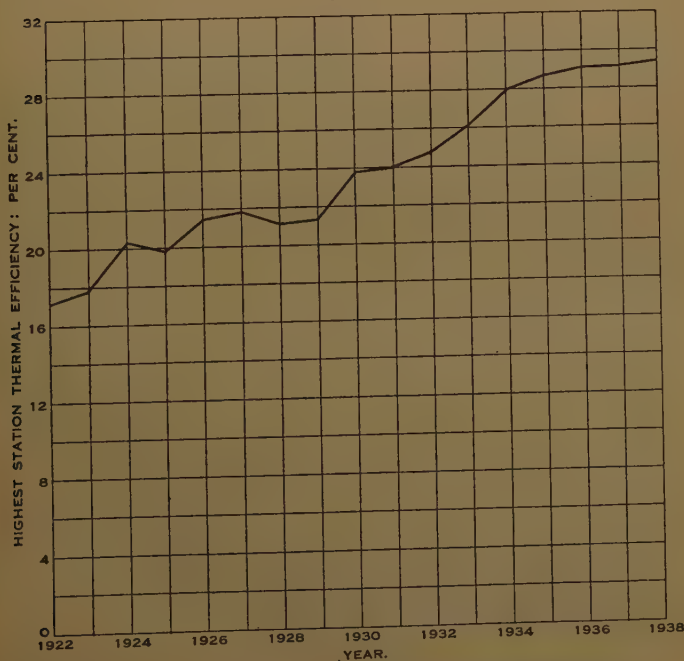
performance. The Carnot cycle could not be carried out in practice by any engine using a working fluid which was condensed during the cycle. The Rankine cycle, however, restored the working medium to its initial state, not by adiabatic compression, but by heating it up again in the boiler. The Carnot cycle gave an unattainable figure, which was the maximum possible efficiency that could be obtained theoretically. It was merely based on the initial and final absolute temperature and was not influenced by what happened between those temperature limits. The standard ideal-cycle efficiency could be defined in a general form as the ratio of the heat utilized as work to the heat supplied. The application of that general expression and the graphical representation of the various cycles in the temperature-entropy chart enabled the Author's valuable formulas to be obtained. They had been in general use for some time as a basis of comparison of theoretical power-station performances.

In reviewing the progress that had been made on the steam cycle since the introduction of the Rankine cycle in 1854, it would be found that proposals had been made by James Weir in 1876 for single-stage feed-heating, whilst proposals for progressive feed-heating had been made by Normand in 1889 and by Ferranti in 1906. The latter had also suggested reheating the steam after partial expansion. In 1913, a higher steam-pressure and temperature and a higher vacuum were introduced at Carville "B" power-station; namely, 250 lbs. per square inch gauge, 650° F. at the stop-valve, and 29 inches vacuum, respectively. Those operating conditions established up to 1923 a new record, with a thermal efficiency of 17·8 per cent. based on units generated. It was then followed by Barton power-station, which held the record for the maximum thermal efficiency for 5 years.

Single-stage feed-heating was put into commercial use for turbine plant in about 1916. The following year a power-plant was carefully considered for a much higher pressure (450 lb. per square inch gauge), for a slightly higher temperature (700° F.), and for an even higher vacuum (29½ inch) with a combination of reheating up to 500° F. and three-stage feed-heating up to 300° F. That gradual step-by-step method of improving the heat cycle compelled engineers to discard all preconceived ideas on the cycle efficiency, and thus to drop the Rankine cycle and to introduce the previously-defined cycle efficiency determined as a ratio of the heat utilized as work to the heat supplied. Those efficiencies had been carefully summarized by the Author for various steam cycles. Both feed-heating and reheating had been largely responsible for bringing the cycle efficiency a step nearer the ideal Carnot cycle. The Author's method of determining the efficiency of the heat cycles with either feed-heating or reheating, or a combination of both, was well known to those who were closely associated with power-station developments. It was of interest to point out that the Author's method of comparison of thermal efficiencies had existed for some time. It was worth while mentioning that two

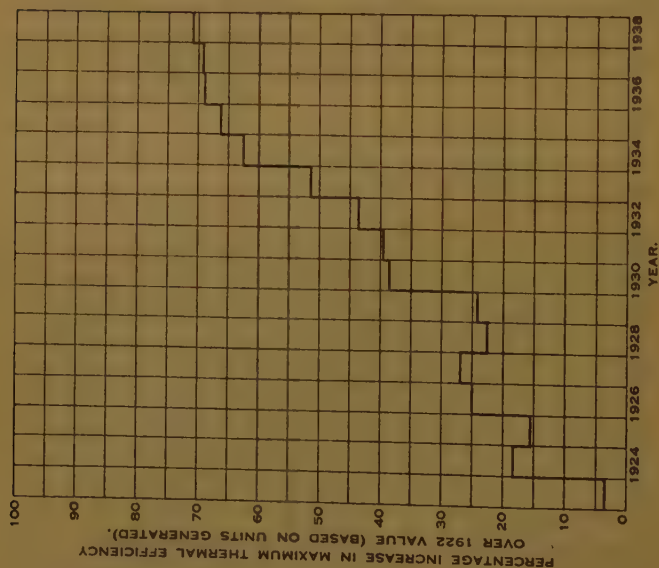
Fig. 7.

LOWEST HEAT CONSUMPTION, BASED ON UNITS GENERATED.

Fig. 8.

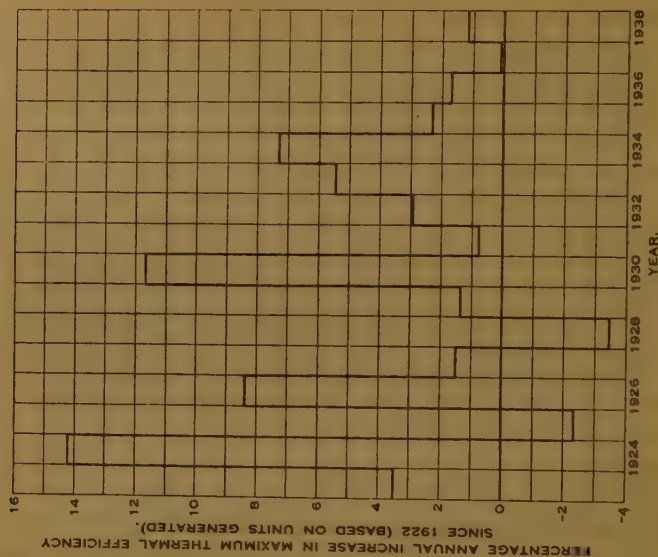
HIGHEST STATION EFFICIENCY, BASED ON UNITS GENERATED.

Fig. 9.



PERCENTAGE INCREASE IN MAXIMUM THERMAL
EFFICIENCY.

Fig. 10.



PERCENTAGE ANNUAL INCREASE IN MAXIMUM
THERMAL EFFICIENCY.

typical cycles, namely those for Carville "B" and North Tees power-stations, were thus compared and published about 15 years ago†. A graphical representation of those cycles was also shown in the temperature-entropy diagram, with the efficiency worked out in detail. Applying the same basis as the Author, Carville "B" station, designed in 1913, gave an ideal cycle efficiency of 34·4 per cent. and North Tees station, which was proposed in 1917 and started up about 1920, gave 40·7 per cent.

In *Figs. 7* and *8* were given the best performances of the year for power-stations in Great Britain for the period 1923–38, based on the Electricity Commissioners' data. The percentage increase in the maximum thermal efficiency for each year relative to 1922, based on the maximum value, was shown in *Fig. 9*, and the yearly rate of increase based on the preceding year was shown in *Fig. 10*.

Dr. E. W. Geyer observed that there was a clamant need for a universally-accepted rule for calculating an ideal standard applicable to modern steam plants in which both tap-off feed-heating and reheating were adopted. The Author's treatment was attractive, but there was one point of difficulty on which his comments would be of interest. In *Figs. 6* (p. 248 §), the heat required to reheat the steam was given as the difference in total heats between the points O and N. In the actual cycle, however, due to frictional reheat, the state point after expansion in the turbine and immediately before reheating lay between N and O, so that the heat necessary to reheat the steam was less than that required under ideal conditions. If, in the example given on p. 248 §, it were assumed that the frictional reheat between E and N was 20 per cent. of the adiabatic heat drop, the total heat after expansion would be

$$1,410\cdot15 - 0\cdot80(1,410\cdot15 - 1,237\cdot15) = 1,271\cdot75 \text{ B.Th.U. per lb.}$$

so that the reheat under those conditions would be $1,430\cdot8 - 1,271\cdot75 = 159\cdot05$ B.Th.U. per lb., in place of $193\cdot65$ B.Th.U. per lb. in the ideal case formulated by the Author. If it were agreed that the reheat should be the same for both the actual and ideal cases, the total heat of the steam after reheating would be $1,396\cdot2$ in place of $1,430\cdot8$ B.Th.U. per lb., and the entropy would then be $1\cdot8048$. The heat supplied would thus be $1,410\cdot15 + 159\cdot05 - 321\cdot7 = 1,247\cdot5$ B.Th.U. per lb. and the cycle efficiency would be

$$\left(\frac{1,247\cdot5 - 538\cdot5 \times 1\cdot3009}{1,247\cdot5} \right) 100 = 43\cdot8 \text{ per cent.}$$

Mr. H. S. Horsman had two principal criticisms to raise: they referred to the standard of comparison (a) for an individual turbo-alternator, and (b) for a complete steam-driven power-station.

† "The North Tees Power Station." *Engineering*, vol. cxviii (1924), p. 58, *Figs. 11, 12, 13, and 14.*
§ *Ibid.*

In connexion with (a), the literature of the subject showed that most authorities favoured a standard based upon a consideration of finite stages of feed-water heating. That preference might be related to considerations which were based upon the optimum temperature of feed-water heating. To exemplify the foregoing, it was only necessary to consider the unusual case of feed-water heating beyond the optimum temperature. If that were done, the efficiency, calculated by the formula which the Author adopted, would be on the increase, but if the efficiency for a finite number of stages were calculated it would be found to be on the decrease. There were, in addition, a number of minor objections to the calculation by infinite stages.

With reference to (b), it was fortunate that the efficiencies as calculated for a finite, and for an infinite, number of stages were roughly proportional, at least when feed-water heating did not exceed the optimum value. The solution for an infinite number of stages was therefore a convenience when dealing with the comparison of complete steam-driven power-stations, as it allowed the ideal efficiency to be calculated from a mere statement of the terminal operating conditions and the temperature of the feed-water.

The definition of the ideal cycle was, however, an arbitrary matter, and so far as steam was concerned it assumed the nature of a compromise between theoretical and practical conceptions. That was as it should be, for if thermodynamic theorems were allowed full play there was danger of arriving at a definition which could be unfair to an actual engine. For that reason most authors insisted upon the maintenance of a strong resemblance between the ideal engine and the actual engine. They agreed to the elimination of certain irreversible processes, such as throttling, friction, reheat, and radiation, but the pressure of the working substance in any part of the cycle was generally allowed to stand.

It was on the bulk of evidence referred to above that Mr. Horsman would refrain from accepting the Author's suggestion to render certain high-pressure feed-heaters reversible, even though such a procedure would extend the usefulness of the formula given on p. 247 §. That formula was strictly applicable only in the saturated region, and it ignored the effect of heat-degradation which accompanied the bleeding of superheated steam. Various methods were known for assessing that loss due to heat-degradation when irreversible feed-water heating took place, and there was therefore no valid reason why such a computation should not be introduced into the calculation of the ideal efficiency.

He had already referred to the arbitrary nature of the basis of comparison, and he would welcome the appearance of a more rigid definition, provided that such a code received the approval of a representative, and preferably an international, body of engineers.

Dr. W. J. Kearton pointed out that the universal employment of regenerative feed-heating in power-stations, and its extensive use in steam-driven merchant vessels, made the formulation of a basis of comparison for thermal efficiency a most desirable object. Those engineers who turned to the Report on Tabulating the Results of Heat Engines Trials for guidance in the matter were bound to be disappointed, for the Report dealt only with the case of single-stage feed-heating, whereas the employment of four and five stages of feed-heating was nowadays common practice.

The Author had done a service, therefore, in attempting to set down a basis of comparison. Before discussing the validity of the suggested basis, reference might be made to a Paper by the late Captain H. Riall Sankey on standards of thermal efficiency*. Captain Sankey's Paper dealt with the case of feed-heating with an infinite number of stages, and in his Paper he published a temperature-entropy diagram for a cycle in which the feed-water was heated right up to the saturation-temperature corresponding to the boiler-pressure, by bled steam which was, in some cases, in the superheated condition. That steam was stated to follow a curve parallel to the liquid line, exactly as the curve RM was parallel to the line BA in *Fig. 6* of the Author's Paper (p. 248 §). Dr. Kearton pointed out in the discussion on Captain Sankey's Paper that the expansion-curve could only be parallel to the liquid line provided that the bled steam was compressed in some way or other. A special reference to his contribution would be found in a short leader in *Engineering*†. Attention was drawn to that leader by Dr. Kearton because he believed that he was the first to suggest the compression of bled superheated steam.

The Author suggested that for complete reversibility the ideal engine would require additional elements to compress the superheated bled steam isothermally to the corresponding saturation-temperature and pressure before actually mixing with the feed-water. Unfortunately, he had not shown in any detail how that might be achieved, nor could it be said that he had really offered a convincing proof that the simple relation which was presented did give the thermal efficiency of the ideal feed-heating cycle.

Dr. Kearton would first consider the compression of the superheated bled steam. *Fig. 11* (p. 346) showed the Mollier diagram for one of the infinite number of isothermal compressions. The point A represented the state point at a certain pressure P_2 and having a superheat t_{s2} . An infinitesimal mass of that steam was extracted from the turbine and compressed isothermally along the constant-temperature curve AB, so that finally it was dry and saturated at P_1 and might be utilized for heating the feed-

* "The Thermal Efficiency of Steam Engines." *Engineering*, vol. cxviii (1924), p. 818.

† *Engineering*, vol. cxviii (1924), p. 866.

§ *Ibid.*

Fig. 11.

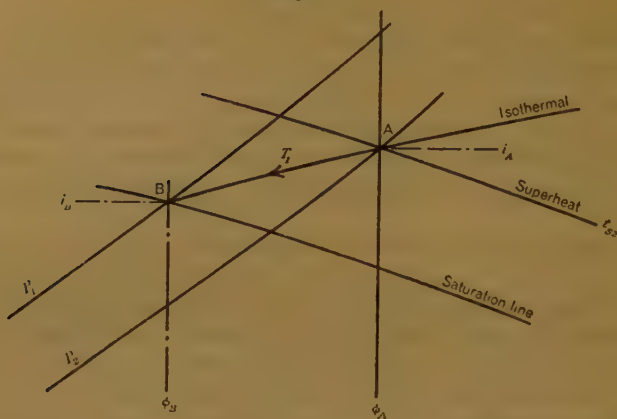
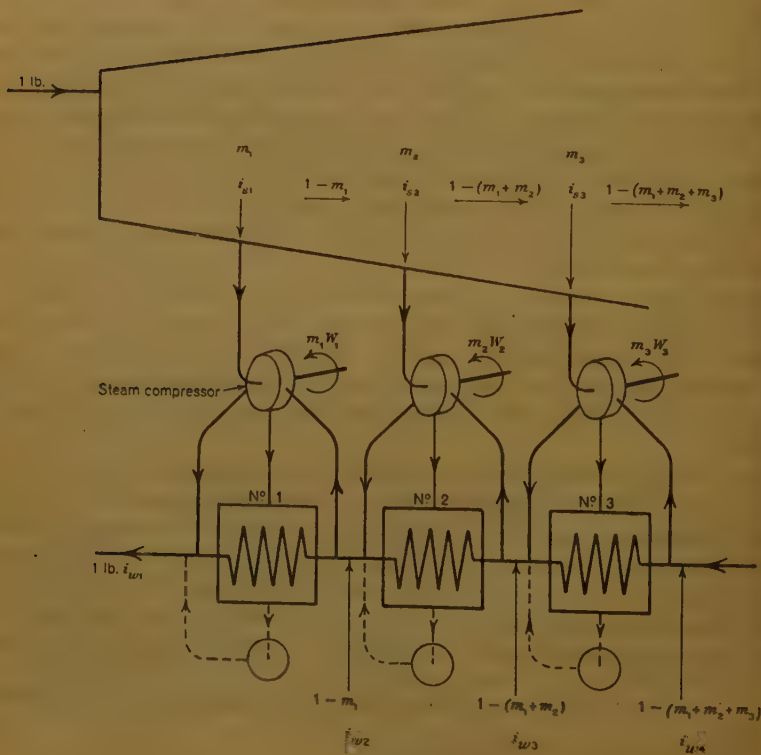
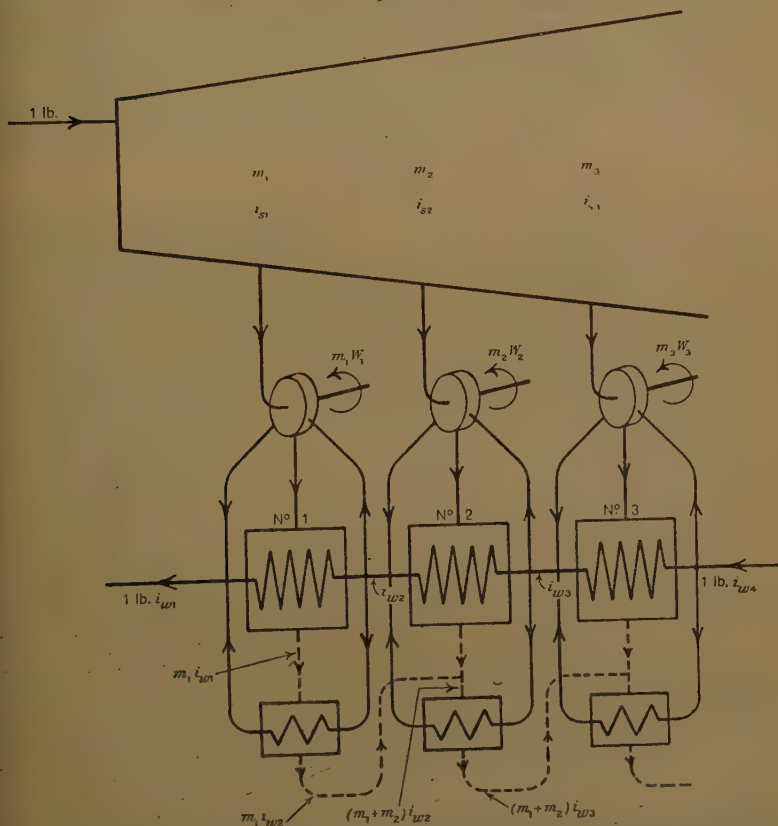


Fig. 12.



water from a temperature very close to the saturation-temperature t_1 up to that temperature. During the compression of the steam a certain amount of heat had to be rejected by the steam to prevent the steam-temperature from rising, and that heat had to be taken up by the feed-water while its temperature was very close to t_1 . Considering the compression of unit mass, let W represent the compression work done in B.Th.U. per

Fig. 13.



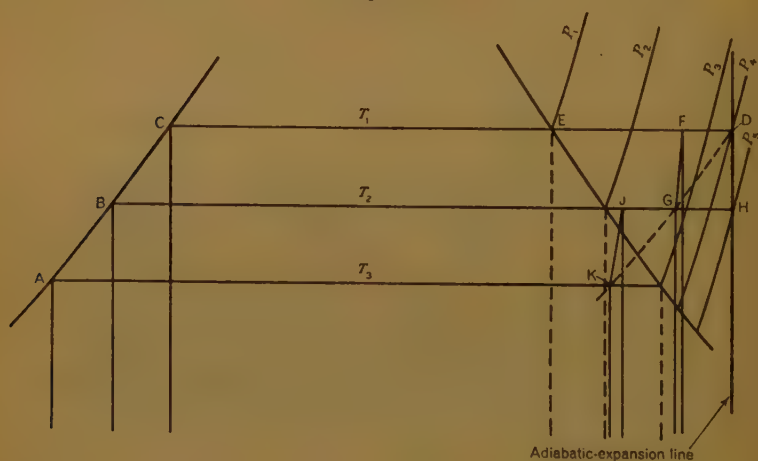
lb. of steam, and let H represent the heat rejected to the feed-water during compression. Then, assuming that the kinetic energies of the steam at inlet to and outlet from the compressor were negligible,

$$\begin{aligned}
 i_A + W &= i_B + H, \\
 \text{or } W &= H - (i_A - i_B) \\
 &= T_1(\phi_A - \phi_B) - (i_A - i_B).
 \end{aligned}$$

The compression and feed-heating might be affected in one of the two ways shown diagrammatically in Figs. 12 and 13. Each showed the extraction

of small masses of steam, m_1, m_2, m_3 , etc., and their compression. A certain proportion of the feed-water would have to be by-passed through the steam-compressor jackets to absorb the heat quantity mH rejected during the isothermal compression. The two arrangements shown differed, in that in *Fig. 12* the condensed steam was extracted by drain pumps and discharged into the feed-pipe after the heater, whereas in *Fig. 13* the drain water was passed through a small auxiliary feed-heater before combining with the drain water from the next heater on the low-pressure side. The heat rejected by the drain water in cooling from the saturation-temperature corresponding to the pressure in the feed-heater to that corresponding to the pressure in the next feed-heater on the low-pressure side was absorbed by a small but definite fraction of the

Fig. 14.



feed-water, and underwent the same temperature-rise as the feed-water in the main feed-heater. In that way the degradation of heat due to a finite number of feed-heating stages was reduced to the lowest practicable limit.

Assuming the employment of the system shown in *Fig. 13*, it might now be proved that the curve FM in *Figs. 5* (p. 246 §) was parallel to the curve BA when an infinite number of feed-heating stages was used, even when the bled steam was in the superheated condition.

Dr. Kearton would next consider two of a finite number of feed-heating stages, arranged as shown in *Fig. 13*. The temperature-entropy diagram was shown in *Fig. 14*. In the first stage, superheated steam was extracted from the turbine at pressure P_4 and temperature T_1 absolute, and was compressed isothermally to pressure P_1 , the state point moving from D

to E. The heat equivalent of the area under DE, that was to say, $T_1 \times (DE)$, was removed by the feed-water which by-passed the heater and flowed through the jackets of the steam-compressor. The latent heat which was represented by the area under CE was transmitted to the feed-water in the main feed-heater.

Let m_1 be the quantity of steam bled off per lb. of steam entering the turbine, and, therefore, per lb. of feed-water. Then the heat-flow in the jacket of the steam-compressor and the main heater, taken together, was equal to

$$m_1 \times (\text{area under CD}).$$

The mass-entropy change was therefore

$$m_1 \times \frac{(\text{area under CD})}{T_1} = m_1 \times (CD);$$

that was represented by DF.

Finally, let the condensed heating steam be considered. It left the main heater at T_1 and was cooled down to T_2 in the auxiliary heater. The heat given up by the condensate was equal to

$$m_1 \times (\text{area under BC}),$$

and might be represented by the area under the curve FG.

There was obviously a slight degradation of heat in each of the three heat-transfers described, but that vanished when the feed-temperature rise in each heater was made indefinitely small.

In the second heater-system, the heat transmitted through the jacket of the compressor and the surface of the main heater was equal to

$$m_2 \times (\text{area under HB}),$$

and was represented by the area under GJ. The condensate entering the second auxiliary heater was $m_1 + m_2$ and, consequently, a greater proportion of the feed might be allowed to flow through that auxiliary heater. The heat given up was equal to

$$(m_1 + m_2) \times (\text{area under AB}),$$

and was represented by the area under the curve JK.

Thus the total area under KJGFD was equal to that under the part of the liquid line lying between A and C. If the number of heaters be increased, the number of steps in the mass-entropy locus KJGFD would also increase, but the area under the stepped locus would still be equal to the area under ABC, both as a whole and in part for any small range of temperature. Ultimately, with an infinite number of feed-heaters, the stepped locus became a continuous curve; then, since for any small range of temperature the area under that curve was equal to that under the liquid line corresponding to the same range of temperature, it followed that the curved locus was bound to be parallel to the liquid line.

Dr. Kearton had also checked, by a numerical calculation for a ten-

stage feed-heating system with a temperature-rise of 10° F. per stage, that the two curves in question were parallel.

There remained the question whether or not it was reasonable to compare the performance of an actual turbine having a few feed-heaters to one having an infinite number of heaters. Such a comparison would always result in an efficiency-ratio lower than the true value, and the turbine manufacturer might, with very good reason, object to such a comparison. Even a comparison of the actual turbine with an ideal turbine having an equal number of feed-heaters was somewhat vitiated by the effect of the irreversible losses which occurred in the actual machine.

There was certainly much to be said for the suggested basis; it represented an ideal which was not beyond the bounds of possibility, and it was eminently simple in form and in use.

Mr. E. L. Robinson, of Schenectady, observed that the Paper appeared to be difficult for American engineers to read because of their unfamiliarity with the terminology used by the Author. Thus, at the outset, the Author referred to the "ideal Rankine cycle" as "completely reversible." Since the ideal Rankine cycle was not generally, in America, regarded as a reversible cycle, Mr. Robinson found great difficulty in understanding either what the Author referred to as the "ideal Rankine cycle" or what he meant by "completely reversible."

The Author proposed for a standard of reference a cycle in which, if a certain feed-temperature had been chosen, the feed was heated by "reversible" extraction to that temperature only; and, if resuperheating were used, the pressure-drop in the resuperheater would be zero. Both the choice of feed-temperature and the decision to resuperheat were highly arbitrary decisions, and the final comparisons would be just as arbitrary. The utility of such comparisons seemed to Mr. Robinson to be of doubtful value.

Some years ago he had prepared some articles on somewhat similar subjects * which led to the conclusion that different standards of comparison were useful for different purposes, and that reference to any standard that fell short of the perfect laws of the art missed by a corresponding amount to reflect useful information.

Thus, a manufacturer was interested in perfecting the internal efficiency of his machine in order that he might meet, and improve upon, his commitments. More important, however, he wished to build for a better cycle of performance with improved steam conditions, conceivably even at the cost of some internal efficiency, since overall performance was what was of interest to the purchaser and the operator.

* E. L. Robinson, "The Margins of Possible Improvement in the Central Station Steam Plant." *Trans. Am. Soc. M.E.*, vol. 45 (1924), p. 644; also in *Engineering*, vol. cxvii (1924), p. 189.

— "Notes on the Comparison of Steam Turbine Efficiencies." *Gen. Elec. Rev.*, vol. 29 (1926), p. 503.

The operator was interested in producing the ultimate product for the least annual expense for equipment and for its operation and fuel cost. He was likely to compare his station fuel-rate with the annual charges on equipment with a lower fuel rate. To select as a standard for reference a cycle which involved one or more arbitrary choices was to restrict attention to comparison with a limited ideal, and all too frequently to enable attention to be drawn to the high degree of perfection with which a very mediocre achievement had been attained.

Professor J. C. Smallwood, of Baltimore, observed that the Paper presented a plausible method of finding efficiency-ratios. There were, however, a number of details of application open to question. The Author emphasized the necessity for comparing actual turbine performance with that of a corresponding ideal engine. That implied strictly reversible processes for the ideal case.

There were three strictly reversible processes: first, isentropic adiabatic; second, isothermal; and, third, heat-transfer in a regenerative system under counter-current flow. The last was not generally and formally recognized as a reversible process, but it might be demonstrated to be such under certain conditions. Heat-transfer from a high-temperature source to a low-temperature medium was, in general, a strictly irreversible process, but if both source of heat and heat medium were fluids varying in temperature, and if heat were transferred under counter-current flow, then the process would be strictly reversible, just as isothermal heat-transfer was reversible, provided that no more than a differential of temperature between hot and cold substances was maintained at all times. That was the first condition of reversibility in the regenerative process. The second was that the heat capacity of the hot fluid had to equal that of the fluid to which heat was transferred.

Those conditions were satisfied by a regenerative cycle on wet or saturated steam as shown by *Fig. 1* (p. 243 §) (excepting only the slight departure at point A, due to feed-pump work). That cycle might be imagined to involve "bled heat" instead of "bled steam", the regenerating heat being abstracted during the steam expansion without altering the weight of steam in the engine. The cycle was completely reversible except for the effect of feed-pump work, which might be taken as negligible, since the accompanying temperature-rise at A was very small.

If, on the other hand, it were attempted to bleed heat from superheated steam in order to pre-heat feed-water up to the evaporation temperature, the regeneration process would not be reversible because the heat capacity of the superheated steam would be less than that of the feed-water. The temperature-fall of the superheated steam would be greater than the temperature-rise of the water for corresponding quantities of heat transferred. It was that difference of temperature that constituted irreversibility.

If an ideal reversible cycle were to be adopted as a standard of comparison, it would follow that an ideal regenerative cycle would have to begin regeneration at the temperature at which the steam became saturated as a result of adiabatic expansion. The Author apparently recognized that fact in the statement (p. 246 §), "... the ideal engine would therefore require additional elements to compress this superheated bled steam isothermally to the corresponding saturation-temperature and pressure before actual mixing with the feed-water. It is convenient to imagine the existence of these. . . ." However, those additional elements for the isothermal compression of bled steam would detract from the useful energy delivered by the ideal engine, and apparently that reduction of available energy was not accounted for. Furthermore, with those additional elements, the cycle would no longer be the simple regenerative cycle, but would be a modified one.

It therefore seemed reasonable to take as the feed-water temperature, at which heat was added by the boiler in the ideal cycle, the temperature corresponding to saturation during adiabatic expansion. That temperature was higher for an isentropic adiabatic than for the polytropic adiabatic of the real turbine. The real turbine began regeneration near the saturation-point of its actual condition curve. The corresponding ideal cycle would begin regeneration at that temperature, and not at the higher temperature of isentropic expansion. Was it fair to compare the efficiency of the real turbine, designed to regenerate at a given temperature, with that of an ideal turbine regenerating at a higher temperature?

In the same way, with regard to reheating, the ideal and the real turbines would reheat at or near the temperature of saturation during adiabatic expansion, but that temperature was higher for the isentropic adiabatic of the ideal turbine than for the polytropic adiabatic of the real turbine. The higher the temperature-level of heat-addition during reheat, the more efficient was the conversion of thermal energy into useful work. The real turbine cycle, adding reheat, by design, from a lower temperature than it could use, was thus disadvantageously compared with the proposed ideal cycle.

Those considerations, and others, had long vexed proponents of methods for calculating efficiency-ratios of bleeder and reheat turbines. In the opinion of Professor Smallwood there was no method yet proposed along conventional lines that was entirely satisfactory. Again, in his opinion the best method (involving a departure from conventional approach) of comparing actual with ideal performance was that involving the "availability function", first proposed by Mr. G. Darrieus*, and later elaborated by Mr. J. H. Keenan†. The latter Paper presented in concrete

§ *Ibid.*

* "The Rational Definition of Steam Turbine Efficiencies." *Engineering*, vol. cxxx (1930), p. 283.

† "A Steam Chart for Second-Law Analysis." *Mechanical Engineering*, vol. 54 (1932), p. 195.

form the application of the method to operating data, and was logical and precise in all its aspects.

The Author of the present Paper was to be congratulated upon presenting a method of calculating an ideal-cycle efficiency, which, whatever was said in opposition, was comparable with the actual turbine-cycle efficiency, and which had the very desirable merit of simplicity.

Mr. C. E. H. Verity observed that it appeared to him that the formula deduced for the calculation of ideal efficiency was not new, and had been published many years ago. In a Paper by Messrs. C. F. Hirschfeld and F. O. Ellenwood*, formulas were given for all the various steam cycles, and included amongst those was the formula given by the Author in his Paper. The formula was also given in an article by Mr. Gerald Stoney on "Standards of Efficiency for Steam Turbines†." Mr. Verity suggested that the formula was already well-known to those who were intimately connected with the question of power-station and turbo-alternator efficiencies, and whilst it appeared to give correct figures when bled-steam feed-heating was carried out in the saturation range, it appeared to be subject to minor errors when that feed-heating was carried out in the superheat field.

The Author, in reply, observed that Messrs. Bottomley, Finnicome, and Verity, in suggesting that the Author's modification to the Rankine cycle was not new, had apparently failed to discriminate between the application of the formulas to bleeding in the saturated steam region of adiabatic expansion, and to bleeding in the superheat region.

Direct bleeding in the superheat region was an irreversible process, however small the increments of temperature. That, as mentioned in the Paper, was because the element of bled steam could not give up its superheat and latent heat at constant temperature, without isothermal compression. The novelty of the Author's argument as compared with the 1926 Interim Report of the Heat Engines Trials Committee, was to assume the process made completely reversible in the superheat region by the addition of elementary compressors, and to establish that when a proper debit for those had been made from the work done in the cycle, the net efficiency of the cycle was still represented by the simple formulas. As given in Messrs. C. F. Hirschfeld's and F. O. Ellenwood's 1923 Paper*, those formulas were not applicable in the superheat region, as they had been careful to point out in paragraph 14 of their Paper. The point had, however, been appreciated by Dr. Kearton, Dr. Geyer, Mr. Horsman and Professor Smallwood. At the time that the Paper was prepared in a preliminary form, early in 1934, for use in the organization with which he was employed, the Author had not been aware of the Papers by Messrs.

* "High Pressure, Reheating, and Regenerating for Steam Power Plants." Trans. Am. Soc. M.E., vol. 45 (1923), pp. 663, 766, and 802.

† *Engineering*, vol. cxxxii (1931), p. 398.

Hirshfeld and Ellenwood and by Captain Sankey †, or of the 1926 Interim Report of the Heat Engines Trials Committee. He was aware that attempts had been made to establish an ideal cycle with feed-heating in infinitely small temperature-steps, and he assumed from the 1927 Final Report of the Heat Engines Trials Committee that that idea had been abandoned due to some difficulty. There could be little doubt now that the difficulty lay in extending the formula to the superheat region.

Mr. Bottomley had apparently used the formula in his 1924 Paper *, secure in the knowledge that the theoretical heat cycle for the North Tees power-station involved so low an initial temperature (650° F.) that bleeding was theoretically possible below the superheat region of expansion for a feed-temperature of 300° F. That was rarely the case to-day, and the formulas as they stood would have been useless.

Mr. Verity would be aware that Mr. W. H. Patchell, who succeeded Captain Sankey as Chairman of the Inst. C.E. Heat Engines Trials Committee, had assisted in the preparation of Messrs. Hirshfeld's and Ellenwood's Paper ‡. Captain Sankey's own Paper † on the subject was given before a combined meeting of several interested Institutions, held in The Institution building, in December 1924, and Dr. Kearton pointed out at the discussion that the formulas proposed by Captain Sankey, similar to those of Messrs. Hirshfeld and Ellenwood, could only be valid in the superheat region if the whole of the steam were expanded to the lower working pressure, a suitable proportion condensed, and the remainder compressed adiabatically with the condensed liquid to give feed-water at the desired temperature. Those processes—adiabatic expansion, isothermal compression, and adiabatic compression—would render the cycle equivalent to the Carnot cycle between the upper limit of feed-water temperature and the lower limit of exhaust-temperature. In as much as no commercial machine to compress the whole flow of working fluid through the turbine had been developed, it was thus argued in the leading article mentioned by Dr. Kearton that Captain Sankey's formulas were inappropriate for superheat bleeding. The ideal cycle had to be based on an ideal engine with surface heaters; that was to say, on the use of regeneration, as in practice.

In considering the present Paper Dr. Kearton at first had some doubt regarding the validity of the formula in the superheat region with the assumptions mentioned by the Author, and he had worked out a practical example assuming initial steam-conditions to be 500 lb. per square inch absolute and 900° F., and he had considered feed-heating between 250° F. and 350° F. in ten stages of 10° F. each, all of which would occur in the superheat region. Dr. Kearton had allowed for isothermal compression, and he had debited that to the work done account. His calculation had

† Footnote (*), p. 345.

* Footnote (*), p. 338.

‡ Footnote (*), p. 353.

confirmed the validity of the formulas. Mr. Verity's attention was drawn to the necessity of carrying out a step-by-step calculation in that manner if results were to be consistent with those obtained in using the ideal-cycle formulas. The Author would like to thank Dr. Kearton particularly for his ingenious proof of the general argument, and for his diagrams, which enabled the process to be visualized as a physical reality with specific values of heat-exchange. Dr. Kearton's contribution was, perhaps, the best reply to the point regarding the debit of compression energy as raised by Professor Smallwood and Mr. Verity.

In further reply to Professor Smallwood, it would be obvious from *Figs. 5* (p. 246 §) that the regenerative cycle consisted essentially of a simple Rankine cycle exhausting heat at a temperature equal to that of the feed-water, to a regenerative cycle working between the feed-water temperature and the condenser-exhaust temperature. As mentioned above, the process of regeneration could be kept completely reversible if superheated bled steam were involved, by assuming isothermal compression of that steam. Thus the lower portion of the cycle became completely reversible in every phase, and was bound to have the limiting efficiency of the Carnot cycle, namely, $(T_2 - T_1)/T_2$. If the heat rejected were represented by a rectangle proportional in area to T_1 , the net useful work of the cycle would be represented by an area bearing the same proportion to $T_2 - T_1$, and it became obvious that the apparent-state line was parallel to the liquid line, and that the simple formulas, with the provision of isothermal compression in the superheat region, became valid for any possible selection of initial and exhaust-steam conditions, reheat conditions, and feed-heating conditions. Professor Smallwood mentioned feed-pump work in reference to *Fig. 1* (p. 243 §), and it was important to emphasize that in using the formulas the heat supplied under feed-heating conditions per lb. of water was calculated from the total energy of the initial steam minus the total energy of the feed-water compressed to the same pressure as the initial steam. There was therefore no departure at point A due to feed-pump work, and the cycle was completely reversible without any qualification whatsoever. The 1939 Callendar Steam Tables did not contain the properties of the compressed liquid other than at saturation pressure, except for a skeleton Table of total heat on p. 8, and the joint authors (Mr. G. S. Callendar and Professor A. C. Egerton, F.R.S.) had been kind enough to furnish the data from which Table I (pp. 356, 357) had been prepared. Those figures were consistent with the saturation figures given in the 1939 Callendar Tables.

An alternative proof to Dr. Kearton's was to consider *Figs. 15, 16, and 17* (p. 358). *Fig. 15* represented a cycle in which the liquid was sensibly heated from A to D, latent heat was added from D to E, and superheat from E to F. The steam was expanded adiabatically from F to H.

TABLE I.—TOTAL HEAT, h , AND ENTROPY, ϕ ,

Feedwater tempera- ture: °F.		Pressure: lb. per sq. in.				
		Saturated	400	600	800	1,000
300	h ϕ	269.7 0.4371	270.3 0.4366	270.7 0.4362	271.1 0.4358	271.5 0.4355
320	h ϕ	290.4 0.4639	290.9 0.4633	291.3 0.4630	291.6 0.4626	292.0 0.4622
340	h ϕ	311.2 0.4902	311.7 0.4897	312.0 0.4893	312.3 0.4888	312.6 0.4884
360	h ϕ	332.3 0.5160	332.7 0.5155	332.9 0.5150	333.2 0.5146	333.5 0.5142
380	h ϕ	353.6 0.5415	353.9 0.5411	354.1 0.5405	354.4 0.5400	354.7 0.5396
400	h ϕ	375.2 0.5666	375.4 0.5662	375.6 0.5656	375.8 0.5651	376.1 0.5646
420	h ϕ	397.0 0.5914	— —	— —	397.5 0.5901	397.7 0.5895
440	h ϕ	419.1 0.6160	— —	— —	419.5 0.6149	419.6 0.6142
460	h ϕ	441.6 0.6404	— —	— —	441.8 0.6395	441.9 0.6388
480	h ϕ	464.6 0.6647	— —	— —	464.7 0.6640	464.7 0.6632
500	h ϕ	488.0 0.6889	— —	— —	487.9 0.6884	487.9 0.6875
520	h ϕ	512.1 0.7132	— —	— —	— —	— —
540	h ϕ	536.8 0.7377	— —	— —	— —	— —
560	h ϕ	562.3 0.7622	— —	— —	— —	— —
580	h ϕ	589.1 0.7874	— —	— —	— —	— —
600	h ϕ	617.2 0.8133	— —	— —	— —	— —

Note.—Linear inter-

* Based on properties of saturated liquid as given in "The 1939 Callendar Steam Table 4 of "Thermodynamic Properties of Steam," by Keenan & Keyes, 1936

ϕ , OF COMPRESSED LIQUID WATER*.

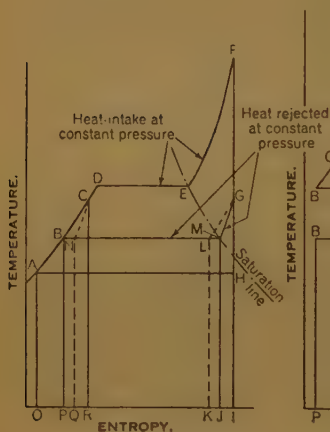
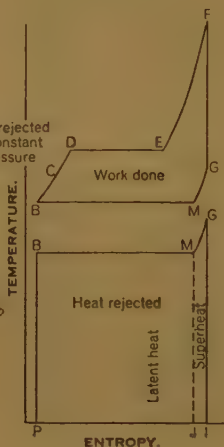
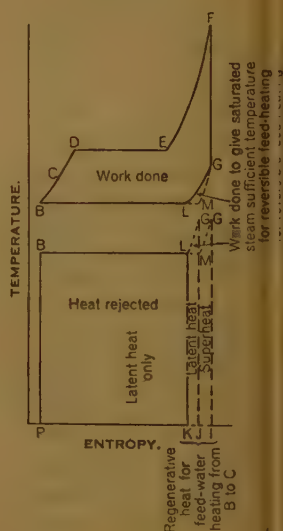
square inch gauge.

1,200	1,400	1,600	2,000	2,500	3,000
271.9 0.4351	272.2 0.4348	272.6 0.4344	273.4 0.4337	274.3 0.4328	275.2 0.4320
292.4 0.4618	292.7 0.4614	293.1 0.4610	293.8 0.4603	294.6 0.4593	295.5 0.4584
312.9 0.4880	313.3 0.4876	313.6 0.4871	314.2 0.4863	315.0 0.4853	315.8 0.4843
333.8 0.5137	334.1 0.5133	334.4 0.5128	335.0 0.5118	335.7 0.5108	336.5 0.5098
354.9 0.5391	355.2 0.5387	355.4 0.5381	356.0 0.5371	356.7 0.5360	357.4 0.5349
376.3 0.5641	376.5 0.5636	376.8 0.5630	377.3 0.5620	377.9 0.5608	378.6 0.5596
397.8 0.5889	398.0 0.5884	398.2 0.5877	398.6 0.5866	399.1 0.5853	399.6 0.5839
419.7 0.6136	419.8 0.6130	419.9 0.6123	420.2 0.6110	420.5 0.6095	421.0 0.6080
441.9 0.6381	442.0 0.6373	442.1 0.6366	442.2 0.6352	442.4 0.6335	442.7 0.6320
464.7 0.6625	464.7 0.6616	464.7 0.6609	464.7 0.6593	464.8 0.6575	465.0 0.6558
487.8 0.6867	487.8 0.6858	487.7 0.6850	487.6 0.6833	487.6 0.6814	487.6 0.6795
— —	— —	— —	511.3 0.7078	510.8 0.7055	510.4 0.7032
— —	— —	— —	535.5 0.7326	534.5 0.7298	533.7 0.7271
— —	— —	— —	560.6 0.7574	559.1 0.7540	557.9 0.7509
— —	— —	— —	586.9 0.7828	585.0 0.7790	583.4 0.7755
— —	— —	— —	614.6 0.8089	612.2 0.8047	610.2 0.8008

polation permissible.

Tables" (Edward Arnold & Co.), corrected for total heat and entropy according to (Chapman & Hall).

Regenerative feed-heating was carried as far as C, by steam bled after adiabatic expansion. It was convenient to consider what might happen in the superheat region only. He would assume that the steam was expanded to the point G, where its temperature was equal to that of the feed-water heated by bled steam, that was to say, of the feed-water at C. A simple Rankine cycle working to point G would then reject heat at constant pressure by following the superheat line GM, and exhausting latent heat still at the same pressure along MB; *Fig. 16* showed the work done, and the heat rejected with that simple Rankine cycle exhausting steam in the superheat region.

Fig. 15.*Fig. 16.**Fig. 17.*

The rejected superheat JMGI could be used for reversible feed-heating of the feed-water between B and C, since it could furnish the quantity of heat QNCR, if the balance of heat required, namely, PBCNQ, were forthcoming from some other source at the same time.

Referring to *Fig. 17*, the balance of heat needed for the process mentioned above could be a quantity of steam with the latent heat represented by the area KLG MJ. The latent heat of the steam was available only at the lower temperature B, and it had to be divided into infinitely small elements of steam to be compressed adiabatically, and then isothermally, transferring work from the "work-done" section of the diagram to the lower section of the diagram as latent heat. A total quantity of heat equal to the area KLG MJ would then be available, together with the superheat already available—namely, JMGI—for regenerative feed heating, with complete reversibility.

It was obvious that in such a case the line LG in *Fig. 15* would be parallel to the line BC, since reversible compression involved no gain or loss in transfer of the energy from one form to the other. The process was equivalent to bleeding at a temperature corresponding to that of the feed-water, with isothermal compression only.

In further reply to Mr. Bottomley, it was noted from *Fig. 11* of his article in *Engineering* † that reheating at North Tees power-station, far from giving any theoretical thermodynamic advantage, actually showed disadvantage as compared with the non-reheating cycle. Why, then, had reheating been adopted? In the article it was suggested that the idea was based on Ferranti's 1906 suggestion of isothermal expansion in the superheat region. There was a profound difference in theoretical thermal efficiency between that and the single-stage reheating cycle adopted for North Tees, and it was extremely doubtful whether Ferranti would have agreed, as suggested in the article, that it would give most of the thermal benefits of his cycle. There could be little doubt that the then high pressure of 450 lb. per square inch gauge was adopted because of its ability to permit the taking in of heat to the cycle at a higher mean temperature. If, as was mentioned, the designers had set themselves an arbitrary limit of 700° F. for the materials then available, with a working temperature of 650° F., then reheating became a necessary evil to avoid excessive wetness and consequent erosion of the low-pressure stages of the turbine. They had no choice in the matter if they wished to benefit from the higher pressure. As far as the Author was aware, nowhere, either in America or in Great Britain, with the exception of Dunston "B" station, had reheating been adopted with any other idea than to facilitate the use of a high operating pressure with a moderate total temperature of steam admission, and to avoid low-pressure-stage erosion. In the latest American Ford plant reheating had been abandoned. Mr. Bottomley was referred to a Paper by Mr. W. S. Burge * for a critical review of the advantages and disadvantages of reheating.

Mr. Bottomley and Dr. Geyer drew attention to the fact that in practice the reheat was considerably less than the theoretical value, partly because of reheating by losses in the high-pressure cylinder, and partly because of extraction before reheating. If the idea were adopted of an ideal engine having no irreversible losses but allowing the pressure and temperature of steam-admission to the high-pressure and low-pressure cylinders, and to the condenser, to stand, as in the original Rankine cycle, there would be no reheat-loss in the high-pressure cylinder of the ideal engine, and no pressure-drop in the ideal reheater. Dr. Geyer would recollect that the ideal Rankine engine envisaged no pressure-drop between the boiler and engine, and it was logical to make the same assumption

† Footnote (*), p. 338.

* "The Present Practical Limits of Power-Station Efficiency." *Journal Inst. E.E.*, vol. 73 (1933), pp. 376-378.

for a reheating engine. The point had been made on p. 247 § that the efficiency of the cycle was not affected whether any bleeding took place before or after reheating, provided that it was done reversibly. Assuming that, an ideal engine with some bleeding before reheat would have greater high-pressure throttle flow and a smaller low-pressure throttle flow than an engine in which the bleeding was done after reheating, but the overall efficiency of each engine would be the same, and the total heat taken in by the ideal boiler and the ideal resuperheater would be the same in each case. The idea of so much heat added per lb. of high-pressure throttle flow, and per lb. of low-pressure throttle flow, was thus liable to be misleading, unless it were emphasized that the cycles had the same theoretical efficiency overall.

Mr. Bottomley apparently considered the maximum theoretical efficiency of the ideal cycle to be an unfair criterion in the case of reheating plants, because of the benefit they enjoyed in working with drier steam. In the Author's view it would be as logical to say that the original Rankine cycle was an unfair criterion because it allowed no more than the true theoretical correction for superheat. Mr. Bottomley had considered the effect of a practical and quite arbitrary superheat correction in comparing Dunston "B" with Barking "B" and Battersea "A" power-stations, and he had apparently overlooked the pressure-drop in the reheater and the profound effect in practice of the size of individual items of plant and operating conditions. Thus, the largest set in Battersea "A" station was twice the size of those at Dunston "B" station, and the boilers were approximately three times the size. Power engineers were well aware of the beneficial effect of size of plant, and of quality of fuel; the calorific value of the latter was roughly 13,700 B.Th.U. per lb. at Battersea, compared with a declared figure of roughly 11,000 B.Th.U. per lb. at Dunston "B" station. The Electricity Commissioners Fuel Returns indicated that Dunston "B" station had a better load-factor and better fuel in 1936 than in 1938.

In further reply to Mr. Horsman, it was assumed that he meant that there was an optimum feed-water temperature in practice if the permissible number of heaters were fixed, say by economic considerations. That was true, but it was no more logical a consideration in the establishment of an ideal standard than an arbitrary limit to the number of stages in the ideal turbine. Mr. Horsman felt that the formula should be modified to allow for irreversible feed-heating in the superheat region, and that had been worked out by Mr. Frederick T. Morse*. The formula, however, became excessively complicated and difficult to apply, particularly to reheating turbines.

In reply to Mr. Robinson, the statement in the Paper read that the Rankine cycle was "completely reversible within itself." That meant

§ *Ibid.*

* "Power Plant Engineering and Design." New York, 1932.

that the cycle was assumed to use the heat supplied to it without friction or loss, and that no cycle could possibly be more efficient with the limitations of heat-supply imposed by the working fluid. As compared with the Carnot cycle, the process of taking in sensible heat and superheat was considered irreversible if the external heat were available at a fixed higher temperature, but if the body furnishing the external heat varied in temperature exactly with the working fluid the process would be reversible, as explained by Professor Smallwood in describing the process of regeneration. The Author felt that choice of feed-water and reheat temperatures were no more arbitrary than choice of initial steam conditions, and of condenser back-pressure and temperature. The Rankine cycle, based on the latter, had been a very satisfactory criterion for 40 years. The Author agreed that the overall thermal efficiency was the thing that mattered, but it was essentially the product of an ideal heat-cycle efficiency and the efficiency of a machine, so that a scientific efficiency criterion for the latter was of fundamental importance.

In further reply to Professor Smallwood, the Author did not agree that regeneration could only begin at a temperature at which the main body of steam became saturated as a result of adiabatic expansion, and that it was reasonable to take that as the ideal feed-water temperature. Most modern plants had feed-heating temperatures higher than that, and there was no question that the process of regeneration could still be completely reversible if isothermal compression were allowed for in the superheat region. The validity of the formula under that condition had been proved by Dr. Kearton.

Monsieur G. Darrieus * stated that (because steam was bled in a series of definite steps in the expansion process) :

"It thus becomes impossible to define unambiguously an ideal cycle of operation, which might serve as a basis for comparison.

"Indeed the various definitions which have been proffered to meet this general case have all been more or less arbitrary, so that the very notion of thermodynamic efficiency thus conceived is affected by a fundamental uncertainty, which has been avoided by no rational criterion as yet proposed."

There was little doubt that Monsieur Darrieus developed that method because of the lack, at that time, of a cycle of comparison for feed-heating in the superheat region and reheating, having the virtue of complete internal reversibility, as the original Rankine cycle had. As compared with the Darrieus procedure, an easily calculated ideal cycle having, in common with the original Rankine cycle, no loophole for argument in definition, greatly simplified scientific analysis of the actual plant performance.

The Author would finally like to draw the attention of all the corre-

* "The Rational Definition of Steam Turbine Efficiencies." *Engineering*, vol. cxxx (1930), p. 283.

spondents to a recent leading article on the subject in *The Engineer* ‡. In that article the need for a completely reversible cycle of comparison was appreciated, including reversible bleeding in the superheat region but the mistake was made of suggesting that allowance for the work done in isothermal compression in the superheat region would complicate the formula. Actually the Author's argument, as confirmed by Dr. Kearton's proof, had greatly simplified the formula. Dr. Stoney's formula mentioned in his article, was essentially that of Messrs. Hirshfeld and Ellenwood, and did not apply to the superheat region, so that its official recognition would probably have failed on that account, as it evidently did in the 1926 Interim Report of the Heat Engines Trials Committee.

‡ *The Engineer*, vol. clxvii (1939), p. 59 (13 January 1939).

CORRESPONDENCE
ON PAPERS PUBLISHED IN
JANUARY 1939 JOURNAL.

Paper No. 5184.

"Improvements at the Royal Docks, Port of London
Authority."†

By RALPH ROBSON LIDDELL, M. Inst. C.E.

Correspondence.

Mr. James Mitchell observed that the increase in height of the keel-blocks of the western dry-dock (p. 287 §) was a much-needed improvement. It would greatly facilitate the cleaning, painting, and repairing of ships' bottoms, and would accelerate the drying of the plating after cleaning, and of the completed paintwork. Headroom was more important than ever, with the ever-growing increase in the beam of ships, and in the size and weight of plates requiring to be handled. Insufficient attention appeared to be paid to the increased cost of dry-dock work due to restricted headroom.

With regard to the tunnel-lining (p. 292 §), the joint shown in *Fig. 8* (p. 293 §) was very elaborate and costly. The lead grummets did not appear to be very suitable for their purpose. In addition to their own cost, they involved the countersinking of the boltholes. Although the segments were made to a tolerance of $\frac{1}{64}$ inch, the bolts had a clearance of $\frac{3}{16}$ inch, and if the whole or part of that were taken up by the holes not coming quite opposite to each other, the grummets would not fit. It was stated on p. 293 § that the boltholes were drilled, but *Fig. 8* (p. 293 §) showed them as cored. Which was correct? If they were drilled, was that method applied also to the countersinking? As it was necessary, apparently, to use a strand of red-leaded hemp, in addition to the lead grummets, it would have been simpler and cheaper to have relied entirely on annular rings of red-leaded canvas, as in a steam-pipe joint, where the conditions were generally much more trying. Although the edges of the flange-joints were welded, that apparently had not been regarded as

† Journal Inst. C.E., vol. 10 (1938-39), p. 283 (January 1939).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

sufficient to produce the required degree of watertightness, and the joints had been filled with injected red lead. Under such circumstances, the machining of the flanges over their full width of 6 inches appeared to be unnecessary. Were the flange-joints vee-welded? If so, how were the vees cut? Notwithstanding its complexity and cost, the joint as a whole did not compare favourably with the much simpler caulked-lead one used in the pipe-subway, especially in view of the effect likely to be produced on the welding by any movement of the lining as a whole. It would be interesting to know why there was such a marked difference between the jointing of the tunnel-lining and that of the pipe-subway, both being subject to approximately the same hydrostatic pressure. Did the tolerance of $\frac{1}{64}$ inch apply to the subway segments, as well as to those of the tunnels? The use of neat cement for grouting outside the tunnel-lining instead of the usual 2:1 mixture seemed somewhat extravagant for such a purpose.

With reference to the cutting away of a portion of the thickness of the roofs of the tunnels, the figure of 1 cubic yard given as an average week's work of a diver seemed low, even allowing for 33 per cent. off delays. Mention was made (p. 296 §) of an experiment with a "hydraulic cartridge" for breaking up the concrete. Some years ago, the ashlar-masonry sill of a dock-passage at Grimsby had been lowered to the extent of 4 feet by drilling holes into which specially-designed hydraulic jacks were lowered. By those means a portion of the masonry had been burst off, and by a repetition of the process the whole sill had been lowered. The operation appeared to have been very successful, but concrete was different in character from ashlar stone. Would the Author give some indication of how the concrete behaved when tested, and why the method was regarded as a failure? Did the term "hydraulic cartridge" refer to a jack such as that used in the above-mentioned work at Grimsby? The puncture of the roof of one of the pipe-culverts by the rock-breaker, and the sticking of the ram in the brickwork, was an awkward occurrence in the passageway of a busy dock. The extraction of the ram was an example of the great value of a powerful floating-crane as a general-utility tool, adaptable to a wide range of emergency operations.

It might be thought that in a dock 3 miles long it would be advantageous to have a deep-water entrance at the up-river end, for the service of vessels using the Royal Victoria basin; the advantage was, however, more apparent than real, since for both entering and leaving vessels the use of such an entrance would involve turning in the river—not a desirable procedure—and an appreciably increased length of course. On the other hand, travelling in the dock probably involved fewer navigation risks, from traffic, currents, etc., than did a corresponding distance on the river.

Mr. E. Fletcher Roberts, of Dunedin, commenting on the Author's statement (p. 291 §) that "Owing to the very flinty nature of the concrete of the old wall the cutting away to form a continuous bearing for the deck and dovetailed pockets for the anchor-beams proved a difficult operation," drew attention to similar trouble which had been experienced during the construction (in 1914) of the crane track referred to on p. 287 §. Dovetailed pockets had been cut for the ends of "raking struts" connecting the crane conduit with the quay wall at 15-foot centres. That work, being executed at that time entirely by hand, had proved so very troublesome that after a certain number of struts had been installed it had been decided to omit that detail of construction. 4-inch diameter holes for draining the conduit had been drilled through the quay wall at 90-foot centres, and those, too, had proved very troublesome. A small pilot hole had first been put through and subsequently enlarged by a second drill, but with hand methods the work had been very slow and irritating.

Some details of the method of design of the 24-inch by 12-inch concrete sheet-piles, referred to on p. 298 §, would be appreciated.

Mr. Roberts also noted, with interest, that the use of a conduit with live wires and plough for feeding the electric cranes had apparently been abandoned in favour of trailing cables, and he would be interested to know how long the conduit and ploughs installed on the north quay of the Albert dock in 1914 had been used.

The Author, in reply, observed that he was obliged to Mr. Mitchell for drawing attention to the error in *Fig. 8* (p. 293 §), where the bolt-holes were described as cored. That had been the original intention, but subsequently it was decided to drill both the holes and the countersinking. The Author was inclined to agree with Mr. Mitchell's criticism of the use of lead grummets and countersunk holes, and with his suggestion that a simpler joint, such as rings of red-leaded canvas, should be considered. The machining of the flanges was an important factor in ensuring the accuracy of the lining, and with the seam-welding of the flush joints it produced a watertight job. The red-lead pump was used for testing the joints, but it was only necessary to inject the red lead into a few of the joints near the taper segments. The simpler caulked joint of the pipe-subway had been adopted because the subway had 13 feet of virgin cover, as compared with about 18 inches of brickwork over the railway tunnels.

The "hydraulic cartridge" referred to was really a jack, and was effective in bursting concrete, but the trials had not produced any uniformity in the depths of the cracks.

Mr. Fletcher Roberts, until he had joined H.M. Forces, had been engaged on the construction by departmental labour of the Albert Dock crane-track and reinforced-concrete conduit, with its collapsible forms. In

spite of precautions the plough-slot had gradually closed in, until in 1928 the ploughs had been abandoned in favour of trailing cables plugged into switch-boxes, which were formed at the side of the conduit at 50-foot centres. The live cables in the conduit had since been replaced by one twin-core 0.5-square-inch paper-insulated lead-covered armoured-type 1,000-volt cable. The 24-inch by 12-inch concrete sheet-piles were reinforced with six steel rods each $1\frac{5}{16}$ inch diameter at 10-inch centres, $\frac{1}{2}$ -inch cast-iron spreaders at 5-foot centres, and $\frac{1}{4}$ -inch binders in pairs at 6-inch centres in the body of the pile.

Paper No. 5198.

“Strata Control in Coal Mines.” †

By HAROLD TAYLOR FOSTER, B.Eng., and MICHAEL ANTHONY HOGAN, D.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Professor S. M. Dixon observed that it was well that civil engineers, who were usually mainly interested in Papers describing great achievements, carried out, in many cases, regardless of expense, should have had brought to them a reminder of what Sir Henry Walker called the “awful toll of accidents” from falls of ground in mines. It was rather humiliating for the engineer to be informed (p. 336 §) that in Great Britain, even in recent years, the annual average number of serious casualties caused by falls of ground in mines had been 1,856. The Authors had ably demonstrated the value of large-scale comparative tests of mine supports and the feasibility of introducing laboratory methods of research underground. On p. 337 § the Authors mentioned the four groups of experiments which they and other investigators had been carrying out, and showed how accurate measurements of the loads on props in mines had been made with the Wazau dynamometer. If the load to be carried by any support were known it should be an ordinary operation for the engineer to provide a suitable support; no doubt the results of those investigations were giving a great impetus to the rapidly increasing use of steel underground. Timber, which had been used because its initial cost was small and because it was so easily handled, was recognized, on account

† Journal Inst. C.E., vol. 10 (1938–39), p. 335 (January 1939).

§ Page numbers so marked refer to the Paper (Footnote (†), above).—SEC. INST. C.E.

of its variable quality, as dangerous, and the lack of economy in its use became evident when compared with steel underground. It would be interesting to have the latest figures giving the quantity of steel underground and its proportion to the coal mined where steel only was used. An approximate estimate made in 1933 * of the amount of steel used in supports in mines had given the figure 640,200 tons. More recently, one of the Authors ‡ had shown that the amount of steel underground at the end of 1937 was nearly 1,000,000 tons, and the scope for engineering principles in the design of suitable steel supports was shown by the fact that, of 14,300 miles of roads in use underground at the end of 1937, at least 5,170 miles were supported by steel. It would be interesting to know something of the trend in prop design. Were rigid or self-adjusting props more in favour? During the initial period of changing over from timber to steel supports, there had been adopted many varying designs, some of which had been suggested by results not completely correlated with the condition of the strata to be controlled. Naturally, steel lent itself to many modifications in design of props, but for economy there should be some standardization.

It was possible to determine, in an ordinary testing machine, the safe load on a steel prop and the action of the prop in resisting loads up to its crushing strength; but for testing supports composed of more complicated structures, such as steel and masonry arches, cogs, and packs, it was recognized early in the researches carried out for the Safety in Mines Research Board that a very much larger testing machine than the usual 100-ton type would be necessary. On p. 343 § the Authors referred to laboratory tests made in a 400-ton machine. In that machine, which had been designed and constructed in the Department of Civil Engineering in the City and Guilds Engineering College, it was possible to test an 8-foot length of roadway supports, 8 feet wide and 8 feet high. The loads applied could be either concentrated at various points or uniformly distributed, and a total vertical load of 400 tons could be applied as well as a horizontal load of 200 tons. When desired, vertical-loading platforms 13 feet long could be used, as in the case of tests on packs.

In describing the experiments on packs underground the Authors drew attention to the very gradual increase in pressure at the beginning, and the abrupt rise after the initial compression. Similar results had been obtained in the earlier tests on large packs in the laboratory †. To predict the exact behaviour of a composite compound structure like a pack it should be possible to perform the difficult task of apportioning

* S. M. Dixon and H. M. Hudspeth, "Steel Pit Props and Mine Arches." Paper at Third International Congress for Steel Development, 1934.

‡ M. A. Hogan, "Steel Supports in Coal Mines." Brit. Steelwork Assn., 1938.

§ *Ibid.*

† L. J. Barraclough, S. M. Dixon, and M. A. Hogan, "Tests on Packs." Proc. South Wales Institute of Engineers, vol. 50 (1934), p. 53 (April 1934).

the special resistance of each component. As pointed out by the Authors, and as noted in the laboratory experiments, the material in the walls suffered severely. Naturally, rough stones built into a wall without mortar would in many cases be fractured under the increasing loads, whilst the settlement of the interior material, which was by no means homogeneous, could not be uniform. It would seem, however, that the results, both in the laboratory and in the mine, might have an explanation different from that given on p. 348 §. The friction between the well-compacted filling and the very rough back of the wall would certainly be considerable. The filling close to the wall would be carried by a slight arching effect so that at first the load on the dynamometer near the wall would be very small. In fact, near the wall the volume of voids in the filling would be much greater than that towards the centre of the pack. Only a very small increase in the load would therefore be expected at the start of the loading. Cross walls would act in a similar manner. Professor Dixon noted that the Authors agreed with Professor Ritson that, although the walls gave an initial, and therefore important, support, their main function was to support the filling. The laboratory tests showed how inefficient the walls were in that way. Although a friable material suitably restrained by wire netting made a successful pack, that did not settle the question of the distribution of the load in packs with stone walls, though it did show that the stone wall support would in general be very uneconomical. It was to be hoped that the very interesting researches begun by the Authors would be continued so that the complete solution of strata control would reduce the number of accidents caused by falls.

Dr.-Ing. Georg Spackeler, of Breslau, observed that the Authors' investigations, which he had studied with great interest, essentially agreed with his own results. That particularly applied to the contention that the load on the supports did not depend on depth but that, on the contrary, the extent of the breaks in the rock strata induced by the working was of fundamental importance for the condition of the supports at the longwall face. Furthermore, the penetration resistance of the roof and floor against the supports was of importance, as the load which could affect the supports was limited by that resistance. The state of stress in rock around a working was very complicated. It was admitted that the weight of the rock mass over an excavation was not on the supports but was carried to a great extent by the coal face and the compressed stowage. In practice the state of stress could be explained by the formation of an arch in the roof over the working, so that, with a longwall face, there would be the pressure of an arch having abutments on the coal face and stowage respectively. The maximum weight, therefore, on the supports in the longwall would be the weight of the rock within the arch, that rock being more or less broken. The question of the magnitude of the latent load on the supports depended on the breaks in the rock within the arch,

on the height of the arch, and on its width. A decisive factor was the distance from the coal face at which the stowage was completely compressed and acted as an arch abutment. On the width of the arch depended the height of its apex. Both of those factors were determined by the nature of the roof strata. Roof control therefore concerned the correct regulation of the pressure arch, and should coincide with the petrographic formation of the roof. In Germany it was customary to differentiate between the strata in the nether roof and in the main roof: the apex of the arch should lie on the lower boundary of the main roof. The faces should penetrate therein as soon as possible after the rock had been disturbed by the working, in order that a state of equilibrium might be formed; that was to say, the stowage should be compressed and an arch formed to provide a safe abutment for the main roof. In comparison with the main problem of exploitation, the strata of the nether roof inside the arch represented a purely practical question. The miner had to have a safe roof over his head. Apart from that, the nether-roof strata might be of service in controlling the main roof. In no case should the new condition of the main roof be disturbed, but when possible it should be utilized. It could be allowed either to subside gradually without fracture, or to cave completely into the working. The main point was that the desired result should assist in controlling the main roof. If the nether roof were allowed to subside without fracture without the main roof following it, then hollows would be formed at the joints and on the boundary of the main roof. As a result, the main roof might suddenly collapse over a large area, causing "bumps" or "weights." It would, therefore, be better to induce fracture in the nether roof and to cause it to collapse. If the fracture of the strata in the nether roof were so great that the resultant fall of rock filled the excavation, the main roof found its abutment on the debris. That was referred to as self-stowage, and with it a gradual subsidence of the main roof was ensured, and "bumps" and main pressure were prevented. In the same way rippings effected at certain distances resulted in a regular subsidence of the main roof, and with that method of control, arches were formed between one ripping and another. Between the rippings, the nether roof collapsed and the apex of the arch lay at the lower edge of the main roof. The thicker the strata in the nether roof the wider the space which could be allowed between the rippings. In favourable circumstances, where the rock could be made to fall without ripping, the latter was unnecessary and self-stowage was practised. The skill of the miner consisted in the correct application of a combination of complete stowage, ripping, and self-stowage. That naturally depended upon a correct estimation of the rock and its properties and behaviour, and especially of the nether-roof strata. The load on the working supports was also greatly dependent on the question of the extent of the strata in the nether roof and to what extent they could be induced to cave in.

Professor Spackeler's observations, compared with the investigations of the Authors, showed that, in general, the same problems arose in the German mining industry as in the British industry.

The Authors, in reply, observed that they were glad to have an opportunity of paying a tribute to the work done by Professor Dixon in the application of scientific methods of measurement to problems of mine support. The latest date for which particulars of the amount of steel in use underground were available was the 30th June, 1938. According to the returns in the reports of H.M. Divisional Inspectors of Mines, the total number of miles of roadway in use at that date was 14,859, of which 6,102 miles were supported on steel. That showed that the quantity of steel in use continued to increase. The majority of the steel props in use in Great Britain were of the rigid type, but there was scope for the wider use of different varieties of yielding props, which was being investigated.

It had been indicated by a later test that the filling near a wall might be, to a certain extent, relieved of load owing to the greater resistance of the wall. A dynamometer set in a roadway pack 2 feet 6 inches from a wall, built of rather large stones, did not record a load for a considerable advance of the coal face. It was realized that the experiments described in the Paper did not furnish a complete explanation of the observed load-distribution across the pack, and further work on that subject was in progress.

Dr. Spackeler's observations were very welcome because they showed that workers on problems of roof control in Germany and Great Britain were thinking along very similar lines. It was to be hoped that useful results would follow from the co-operation of workers in different countries on the important problems governing safety in mines.

Paper No. 5177.

"An Experimental Study of the Voussoir Arch." †

By PROFESSOR ALFRED JOHN SUTTON PIPPARD, M.B.E., D.Sc., M. Inst. C.E.,
and RONALD JOSEPH ASHBY, M.Sc. (Eng.), Stud. Inst. C.E.

Correspondence.

Professor R. V. Southwell thought that the most striking feature of the Paper was the close agreement of its theory with the experimental curve F in *Fig. 9* (p. 895 §), especially when consideration was given to observation (7), p. 400 §.

If the voussoirs could be treated as rigid and indefinitely strong, then (as had been already shown *) a statical theory, on the basis of mechanical instability occurring in what was effectively a four-bar chain, would be adequate. There would seem to be little doubt that a theory of that kind served to explain all the essential features of the problem, but it should be remembered that such a theory presumed a knowledge of the points at which "hinging" could occur. That knowledge was available on the understanding that the voussoirs were rigid and had plane faces: "hinging" could then occur only at the extrados or at the intrados. If, however, a procedure were adopted such as the insertion of pads between adjacent voussoirs, then "hinging" could be made to occur anywhere. If such pads were soft and of some extent, then it was certain that the effective hinge would, in every case, lie at some point between the intrados and extrados. If a soft rubber pad were fitted over the whole plane face of the voussoir, then, as instability approached, the stress in the rubber would be either zero or compressive, with a maximum at the face of the arch; the effective hinge would again lie between the intrados and extrados, since the rubber would not permit line-contact between voussoirs. If, furthermore, it were assumed that the rubber could only sustain a limited stress without failure, then, as instability approached, there would be a further circumstance making for an inward movement of the hinge; a finite depth of hinge would be necessary, given by the quotient of twice the thrust by that limiting stress. That was on the assumption of a linear distribution of stress across the depth of the pad, and, on the same under-

† Journal Inst. C.E., vol. 10 (1938-39), p. 383 (January 1939).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

* A. J. S. Pippard, E. Tranter, and L. Chitty, "The Mechanics of the Voussoir Arch." Journal Inst. C.E., vol. 4 (1936-37), p. 281 (December 1936). *Discussion* in Journal Inst. C.E., vol. 6 (1936-37), p. 5 (June 1937).

standing, the effective hinge would lie at one-third of that depth from the heavily-loaded edge. When the problem was visualized in that way, it seemed clear that finite strength would entail an inward displacement of the hinge, becoming greater as the thrust increased; that in turn would entail instability at a lower load than before.

Assuming a figure for the limiting stress, it would appear possible to give that argument quantitative form and to calculate the load at which mechanical instability would occur. Professor Southwell suggested that the converse calculation should be attempted, to see whether the results of *Fig. 9* (p. 395 §) were consistent with some reasonably uniform strength-value for the lime-mortar, and if so, what that value would be. It would be highly satisfactory if the result should emerge that *Fig. 9* could have been predicted on the basis that a definite limit to the crushing strength of the mortar implied a definite inward movement of the hinge when subjected to a given thrust; for then (because the inward movement increased with the thrust) there would again be a definite stability-limit which would, however, be less than that calculated on the assumption that the voussoirs had infinite strength and touched one another directly.

The Authors, in reply, observed that the point raised by Professor Southwell had been considered in some detail, and there was little doubt that if the jointing were formed of a material whose elastic properties were definite it would be possible to calculate, with very good accuracy, the real position of the pins. A small model-arch had actually been made in which the jointing material was replaced by rubber pads whose positions could be varied, but it had not been considered worth while to carry that work very far. When the original research had been planned it had been intended to carry out some tests with rubber pads about $\frac{1}{4}$ inch thick replacing the mortar joints, but unfortunately that set of experiments had not been done.

The arch was, however, still available, and it might be possible during the course of next year to complete the original programme and to throw further light on the location of the "pins" in the joints under the conditions suggested by Professor Southwell.

§ *Ibid.*

CORRESPONDENCE

ON PAPERS PUBLISHED IN

FEBRUARY 1939 JOURNAL.

Paper No. 5203.

“The Conditions of Engineering Contracts.” †

By EDWARD JOHNSON RIMMER, M.Eng., B.Sc., Assoc. M. Inst. C.E.,
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Correspondence.

Mr. J. S. Alford wished to emphasize certain points which had been only briefly referred to by the Author.

Under the Association form, all variation orders had to be signed by the Engineer, and no variation could be made by the Contractor without such an order. Under the same form the Resident Engineer had no authority to issue variation orders. Circumstances sometimes arose under which that procedure was difficult. Some relaxation was needed for the authorization of emergency works, more particularly when the headquarters of the Engineer were far removed from the site of the works. In a set of General Conditions, which had been drafted by the late Mr. A. A. Hudson, the Contractor had to produce, at the time when the accounts were made up, orders signed by the Engineer for all alterations. The Engineer was able, under those conditions, to issue orders for extra works retrospectively. The case of *Buchan v. Feltham Urban District Council* illustrated that point. If the Employer should object to such a provision, he should secure protection in the Agreement for Employment and should not seek it in the General Conditions.

Referring to arbitration, he strongly supported the Author's view that that should usually be undertaken by some party other than the Engineer. On small contracts, however, the method of arbitration by an outside person was too cumbersome and expensive and the Engineer might then be the sole arbitrator. He hoped that, in such cases, it would be found possible to modify the usual clause of appointment so as to give the Engineer complete authority over all engineering matters, including payments arising therefrom, whilst reserving to the Contractor the right to take legal proceedings, if he considered the contract documents did not properly represent the work which he had priced. The Contractor could, in that

† Journal Inst. C.E., vol. 11 (1938-39), p. 3 (February 1939).

way, avoid the necessity of making personal and unpleasant allegations against the Engineer in order to obtain a locus when, at the most, a mistake had been made.

Referring to the billing of the general obligations, Mr. Alford considered that it was useful to set out by numbers the whole of the clauses in the General Conditions and Specifications in bill No. 1 of the quantities, thus giving the Contractor an opportunity to price all the various obligations separately by clauses.

On p. 20 § the Author referred to the desirability of putting upon Sub-Contractors the obligations which the Contractor had assumed. In a civil engineering contract where there was a civil engineering sub-contract that was practicable, but where the Contractor had to make a sub-contract for the supply of machinery it was not, as a rule, practicable. In such cases the Contractor might be given an item in the bill of quantities to enable him to price the obligations which he could not transfer.

Mr. Fred Lavis, of New York, observed that the forms of contract in the United States since 1914 had tended to eliminate the assumption by the Contractor of undue or unforeseeable risks. In the earlier forms of contract, especially those for the construction of railways, the clauses giving the Engineer complete and final authority were so stringent that full compliance with both the literal aspects of the contract and with what the Engineer might decide, might easily have completely ruined the Contractor. Those forms of contract had been built up in course of time by engineers and lawyers as a defence against sharp practices by unscrupulous and not entirely responsible contractors during the era of expansion. Any Contractor who undertook a contract under such a form was, therefore, almost entirely at the mercy of the Engineer, and the profession could well be congratulated that its members generally exercised their powers fairly and with wisdom.

Dating from the beginning of the twentieth century, however, with many more responsible and established contracting firms, who had their own responsible engineering staffs and competent direction, the tendency had been to develop forms of contract suitable to agreements between more or less equally responsible and competent contracting parties. In tunnelling, for instance, the Contractor might be paid for "over-breakage" for a specified distance beyond the line of minimum section "neat line", but all "over-breakage" beyond that zone was not only for the Contractor's account but he had to back-fill carefully the space with suitable material. That sort of provision still required careful work by the Contractor but recognized the impracticability of hewing to a given line in a rock tunnel, and minimized the risk for ordinary reasonable "over-breakage."

In the case of foundations, even where careful borings had been taken,

§ Page numbers so marked refer to the Paper. Footnote (†), p. 373.—SEC. INST. C.E.

the Contractor, whilst not relieved of all responsibility or troubles due to lack of experience or carelessly conducted work, could be relieved of some of the responsibility for unusual and unforeseen developments which might involve expense or increased time (under a time-penalty clause).

The courts of the United States had been very jealous, under the terms of the English common law, of guarding the rights of Contractors who had cause of complaint against what were considered unfair decisions by the Engineer, even in contracts where it had been agreed that the decision of the Engineer should be final.

Mr. Lavis emphasized that where the risks were modified for the Contractor, not only was the contract a much fairer instrument, liable to be upheld by the courts, in case of need, but also lower prices usually resulted.

The Engineer had, of course, to protect himself and his client fully, and the forms of contract minimizing the risk to the Contractor probably required much more care and skill in their preparation than did contracts of a more arbitrary nature. That care and skill, however, was what the Engineer's client was entitled to expect (together with the resulting lower prices), and should be furnished if the Engineer were fully competent and careful.

Mr. E. J. McKaig observed that the impression left by some of the impractical and one-sided conditions frequently embodied in engineering contracts was that they might be due to the intervention of the legal mind. That impression was supported by the fact that, more often than not, the more unreasonable the terms the more legal the phraseology. Sir Lynden Macassey suggested that conditions which savoured of sharp practice might be due to the too-willing assistance of lawyers. Whether or not that were true, there were many quite usual conditions which would not bear examination from the point of view of common sense and equity.

Referring to the Contractor's liability for all damage (p. 13 §), the Author stated that there was such a heavy risk that it seemed surprising that business men had been willing to take it. In the quotation from Lord Justice Bowen's judgement (p. 24 §) similar sentiments were expressed. The inference was that the Contractor entered into the undertaking fully aware of the magnitude of these risks. That was, however, not quite true: usually, before the Contractor could examine the documents governing the contract, he had to pay a deposit (returnable on receipt of a bona-fide tender). It was not, therefore, until he was involved to the extent of the deposit (small though it might be), and so was considered as a potential Contractor, that he was aware of some of the conditions, which, if enforced, could easily make the position impossible so far as any profit was concerned.

Mr. McKaig had in mind two Specifications, Conditions of Contract, and Bills of Quantities, for contemporary works of considerable size.

They were issued by different engineers, and the works were for public authorities made possible by unemployment grants. The one was completed without any serious dispute; in the other, disputes arose quite early as a result of the more or less impossible terms, and ended in a long and costly arbitration.

It might be said that, since the first case was satisfactorily completed under those conditions without dispute, there could be little wrong with them. They had functioned satisfactorily. In the second case, however, it might be said that although the disputes were based on certain conditions of the contract, they arose in fact, from the temperament and character of the Contractor. He was a litigious person and would have found cause for dispute in any case: was that so?

In the first case the Bills of Quantities described a large quantity of Portland-cement concrete. The Specification and General Conditions called for the British Standard Specification for Portland cement. Further on in the same document was a clause: "Rapid hardening cement shall be used if directed by the engineer." That sounded quite simple, but what did it mean? In making his tender, the only item the Contractor had to price was Portland-cement concrete; but he was under the liability of having to use a cement which cost more per ton, and could not be stored for the same length of time as ordinary Portland cement. Was he to quote for the more expensive, rapid-hardening cement, which he might never be called upon to use, or was he to quote on the basis of the cheaper cement and take the risk of a loss throughout the work?

The quantities in that case clearly described the concrete required, but the Conditions gave the Engineer the right to vary that description, without making any provision for the Contractor to recover the increased cost of the variation.

A further clause stated: "The Contractor shall guarantee the stability of the works and be responsible for accidents from whatever cause." That might appear reasonable except for the words "whatever cause." If the Contractor were to be responsible for anything that might happen, whatever the cause, it was necessary to examine the various causes of instability of works. Omitting matters such as erection, and machinery, and the sort of accidents that, unfortunately, did happen from time to time and were risks which the Contractor always carried, the probable causes of instability might be taken as the following:—

1. The design of the works being inadequate to the conditions or duty for which they were designed.
2. The type and quality of materials specified being unsuitable and inadequate to the design.
3. Bad workmanship.
4. Failure of the site.

Of those four items, Nos. 1, 2, and 4 were beyond the control of the

Contractor. He neither designed the works, specified the materials, nor chose the site. Workmanship, however, was the Contractor's responsibility, although that was usually very carefully watched and controlled by the Engineer and his inspectors. Nevertheless, if any failure or damage were to arise because of bad workmanship, it was reasonable to make it the liability of the Contractor. The Contractor engaged on that contract, on being asked whether he realized the grave liabilities embraced by the General Conditions of Contract, stated that he was quite aware of the unreasonableness and absurdity of many of the terms; since, however, a number of other Contractors had willingly tendered to them he had done so and he felt sure that there was no intention of enforcing or even attempting to enforce them. If the Contractor were correct in his surmise, there was no justification for such Clauses appearing at all. The contract which contained many such conditions carrying heavy risk was completed without dispute. It would have to be admitted, however, that the work was attended by a certain amount of good fortune.

Mr. McKaig then referred to the second case in which disputes did arise. One of the claims was based on the clause which stated:—"The Contractor shall satisfy himself or shall be deemed to have satisfied himself as to the nature of the ground, dimensions, levels, character and nature of sewers, drains, etc., which can in any way influence his tender, and no claims for extra works or otherwise will be allowed in consequence of any incorrect information on these points or any inaccuracies with reference thereto which may appear on the drawings or in this specification. Nor shall the Contract be nullified in consequence of any error, incorrect information or inaccuracies." Considering first only the part referring to the nature of the ground and surroundings, that might be reasonable for certain works, but it would be dangerous to make it a hard and fast rule. It could only mean that contractors tendering should make a survey of the site, both geographical and geological, regardless of the fact that before the drawings and specification could have been prepared, the Engineer (who was presumably the author of the clause) had necessarily undertaken such a survey. The necessary investigation might be quite a small matter, but equally it might mean costly and lengthy operations such as boreholes and trial pits. Then it had to be considered whether there was sufficient time, between the invitation to tender and the closing date, to carry out any exploration that would yield reliable information. Such a survey, especially one involving excavations which might (in the same way as the contract Mr. McKaig had in mind) affect a main thoroughfare, would be impossible if there were a number of contractors wishing to tender. Such a project became ridiculous when it was considered that the information sought was already in the possession of the Engineer.

A clause of that kind suggested that the Engineer was attempting to unload the burden of any possible shortcomings of his design on to the Contractor. The Engineer might even be aware that the final cost would

be very much greater than the sum indicated by his drawings and specification, and therefore that much greater than the amount which he had led his employers to expect. The appearance of such a clause would at once arouse suspicion that the conditions of the site might not be as shown on the drawings, and were the Contractors, before tendering, to make investigations at the site proving it was not of such nature as the drawings would lead them to suppose, what course was open to them? Could they amend the drawings, and introduce appropriate items into the quantities to meet the case? Or could they, on the other hand, tender in the prescribed form, knowing full well that the work could not be performed to them?

The latter part of the clause referring to incorrect information and inaccuracies might, of course, be a safeguard to provide against accidental errors and oversights, but there was nothing to show that deliberate misrepresentation was excluded.

Following the clause that the Contractor should satisfy himself as to the nature of the ground, were two stipulations that were more or less corollaries to it:

1. "The Contractor shall reconstruct at his own cost any work he may have erected upon an insecure or insufficient foundation."
2. "No foundation shall be placed until the engineer has inspected the excavation and given his approval in writing as to its suitability, but even that approval will not relieve the Contractor from his responsibility for the stability of the work."

However practical those stipulations might be, there appeared to be some redundancy about them. If the Engineer's approval in writing did not relieve the Contractor of any liability, then at the best it was a valueless document and might be dispensed with.

In the contract to which those stipulations related, disputes did arise, and one claim was contested on the basis of the clauses quoted. During the course of the work the Contractor found the conditions of the ground at one part of the site to be such that it was obvious that the structure, as designed and shown on the contract drawings, could not remain stable for long, if in fact it would be possible to construct it. The information was at once communicated to the Engineer with suggestions showing how the difficulty could be met. The suggested amendment involved some additional expense. The Engineer, however, would not give any order for the extra work or variation and relied on the clauses which subsequently caused so much controversy.

After some difficulty that part of the work was completed, and, as had been obvious, within a few weeks it collapsed. The Contractor considered he had fulfilled his part of the contract since he had warned the Engineer of what would happen. The Engineer, however, pointed out that under the terms of the Contract the Contractor was responsible for the site being suitable for the work, that was to say, no extra payment would be made

for anything that might be necessary for the stability of the work. The matter went to Arbitration. The Arbitrator, who was an engineer, awarded against the Contractor, who had to construct the work to an amended design. It was therefore not quite true to say that some of the harsh and one-sided stipulations were a provisional defence against the unscrupulous and dishonest contractor, and would not in the ordinary way be enforced, or that they were not intended to be interpreted so that an unfair advantage over the Contractor could be obtained.

Mr. McKaig offered the information that he himself was not a contractor: the documents quoted were obtained through the good offices of the engineer in the one instance, and from the contractor in the other.

Mr. John Pollock, of Nairobi, suggested that there should be compiled a "Standard General Conditions of Contract", applicable to all contracts. The conditions for each individual contract could be modified by the addition of clauses for "Special Conditions" which would be the responsibility of the Engineer-in-Charge. Referring to the amount of time spent in petty work such as clearing up the site after the main structure was completed, he thought that the time clause should take account of the "Substantial Completion of Contract."

It seemed unreasonable to ask contractors to carry out works 50 per cent. or more over the schedule quantities at schedule rates: from 15 to 20 per cent. was a fair limit to place on quantity variations; anything over that limit should be subject to a revision of the schedule rate.

Mr. Pollock suggested that the appointment of the Engineer as arbitrator was not fair to the Contractor, since the Engineer, by virtue of his position, would find it difficult to judge any dispute with an unbiased mind. The institution of a "Standard General Conditions of Contract" would indicate to many Engineers that their legal position was not as strong as they had hitherto believed, and so would reduce the number of disputes which required arbitration.

Mr. E. G. Walker supported the Author's views (pp. 6 and 7 §) regarding temporary works. In the majority of cases the aggregate amount of practical experience of execution of works available to the Contractor was greater than that available to the Engineer, although the latter was in a better position to assess the ultimate results which he wished to attain. Unfortunately, in civil engineering contracts the two functions of design and construction could not always be separated completely, and details of design frequently had to be modified to suit exigencies of construction. The responsibility and financial risk in the execution of the works, however, fell upon the Contractor. The Engineer who specified in too much detail how the works were to be carried out might run needless risks by so doing, in that he was taking on himself responsibilities which the Contractor was paid to carry. The freest possible hand should be given to the Con-

tractor, reserving only to the Engineer that over-riding control which it was essential he should possess, though not always exert.

The Author's suggestion for discriminating more clearly between a "bill of quantities" and a "schedule of prices" was good and should be adopted. Unfortunately, works varied very much in character, and individuals differed very much in their views as to what constituted an adequate statement of the details of the work; to obtain a general standardization of practice was likely to prove difficult. At the same time there was no reason why the Author's definition of the two terms "bill of quantities" and "schedule of prices" should not be adopted. An incidental advantage of a rigid demarcation between the two would probably be that in the preparation of his contract documents, the Engineer would, by having his attention directed to the two classes of price covered by them, be more likely to prepare a more complete schedule and thus avoid the not infrequent difficulties, in the absence of basic data, of having to adjust prices purely by a process of bargaining.

The Author drew attention to the difficulties arising from the variations in practice in regard to pricing preliminary items. Individual contractors often differed widely in their treatment of preliminaries and so added another complication to the Engineer's task of comparing tenders. In many contracts, particularly those emanating from public authorities, there was a considerable elaboration of preliminary items, though often a few of those items were actually priced in the tenders. The question arose of how far many of those items were necessary in the bill of quantities. A proper specification and set of contract conditions should describe fully the circumstances under which the works were to be executed and the responsibilities of the Contractor in relation thereto. In many cases, therefore, it would appear unnecessary to refer to them again in the bill, particularly as many contractors preferred to spread overhead charges over the construction items of the bill rather than to price them separately.

There existed far too great a tendency for Employers to expect Contractors to carry risks which were the legitimate risks of the Employers. The Author referred to that matter in some detail on p. 13 §. His solution for the difficulty that might arise from the materialization of a risk of that sort was the issue of the extra-works order by the Engineer. Whilst that might be an immediate practical solution, it was one that, in most cases, placed the Engineer in an arbitrary position in which he had to assess the liability involved and to decide whether it should be borne by the Employer or Contractor. Such a position was easier for an independent practising engineer who, himself, had a contract with the Employer to design and supervise the construction of the works, than it was for an engineer who was the paid servant of the Employer. The present-day tendency for expenditure on civil engineering works to come more and more within the

control of public administrations or very large commercial combines, all of which employed their own technical staffs, was resulting more and more in the administration of works contracts being in the hands of employees of the Employer. That condition did not arise when, as was the practice in the earlier days of civil engineering, the contract was administered by an engineer practising independently. Such a practice was a far better way of maintaining a balance between the Employer and the Contractor. A contract which could be administered equitably under those circumstances might need considerable alteration to provide for an equal degree of fairness under the more modern conditions. It was remarkable that, whilst modern arbitration law provided so very carefully for the complete independence of the Arbitrator, there were so many engineering contracts which left the settlement of large possible differences in the hands of one of the parties acting through his own employee.

Mr. Walker endorsed the Author's view of the desirability of attempting some closer standardization of the form of civil engineering contracts than now existed. Admittedly it was a subject which had its full measure of difficulties. Already a certain amount of work had been done. The Author referred to the Conditions drawn up by the Association of Consulting Engineers. The Institution of Electrical Engineers had drawn up a standard for electrical contracts and the Institution of Mechanical Engineers had the subject under consideration in relation to mechanical engineering. The standards of the Royal Institute of British Architects had been in general use for building work for a number of years. There was every reason for making a co-ordinated study of the subject of contracts for civil engineering works and for endeavouring to evolve standard forms applicable to the various conditions under which such contracts were carried out. The range of contracts embraced under the title of civil engineering, using the term in its more restricted sense, was greater perhaps than that of mechanical and electrical engineering contracts, which normally had to cover only the supply and installation of machinery and plant, and therefore the subject might well be found to require more detailed study.

The Author, in reply, emphasized the importance of the Engineer's right to order extra work retrospectively. The case referred to by Mr. Alford clearly showed that unless the Engineer had power to implement an oral agreement with the Contractor by a subsequent order in writing, the Employer might escape payment for extra work, or might refuse to be bound by the Engineer's certificates on the ground that the order given was not in writing at the proper time. The right of the Engineer to waive merely technical conditions precedent to payment would appear to be reasonable.

The question of the nature and extent of preliminary items of Bills of Quantities was referred to by Messrs. Alford and Walker. The Author considered that the Bill should not attempt to restate the general obliga-

tions and liabilities set out in the General Conditions and Specifications, but should merely set out those items which necessitated work (lighting, watching, cleaning up, etc.) or a payment by the Contractor (insurances, damages for surface disturbances, fees, dues, etc.). General liabilities were not, in his opinion, the proper subject of item charges in the Bill, though the Contractor might be given the opportunity, if he thought fit, of inserting items of a general nature. Otherwise, they should be proportionately distributed throughout the items. The setting out of a series of clause numbers from the General Conditions and Specifications could not make the Contractor's liability any more definite, although a note to the effect that the prices in the Bill were to cover all obligations and liabilities set forth in any of the Contract documents was perhaps desirable.

Paper No. 5179.

"The Properties of Composite Beams, consisting of Steel Joists Encased in Concrete, under Direct and Sustained Loading." †

By PROFESSOR CYRIL BATHO, D.Sc., M. Inst. C.E., STANLEY DALE LASH, Ph.D., Assoc. M. Inst. C.E., and REGINALD HERBERT HONLEY KIRKHAM, B.Sc., Stud. Inst. C.E.

Correspondence.

Dr. Kálmán Hajnal-Kónyi observed that it was interesting to compare the total moments, at failure, for similar beams in the two groups with mix (a) and mix (b) in Part I of the Paper. It was stated (pp. 63, 64 §) that the specimens of the second series were made with a stronger mix of concrete and a greater cover over the compression flange of the joists; otherwise Nos. 8 and 9 were similar to the beams in the first series. It was, therefore, remarkable that the total bending moment at failure (Table II, p. 78 §) in beams Nos. 1 and 2 was 527,000 and 579,000 lb.-inches respectively (average: 553,000 lb.-inches), whereas in beams Nos. 8 and 9 it was only 488,000 and 501,000 lb.-inches (average: 494,500 lb.-inches). That was

† Journal Inst. C.E., vol. 11 (1938-39), p. 61 (February 1939).

§ Page numbers so marked refer to the Paper. (Footnote (†) above).—SEC. INST. C.E.

a decrease of about 10 per cent. although the working moments according to the Code of Practice were in the opposite relation :—

Beam 1 : 140,500 lb.-inches	} average : 144,000 lb.-inches.
Beam 2 : 147,500 lb.-inches	
Beams 8 and 9 : 198,000 lb.-inches.	

If the values supplied by the Code of Practice were reliable, an increase of 37 per cent. should have taken place, instead of a reduction of 10 per cent. The discrepancy was still greater in beam No. 12, where the addition of two bars 1 inch diameter, in compression, raised the working moment to 285,500 lb.-inches (that was to say, to twice as much as the average for beams Nos. 1 and 2), whereas the total moment at failure, 540,000 lb.-inches, was less than the average for beams Nos. 1 and 2. Those deviations between working moments and total moments at failure were demonstrated by the great variations of the load-factors on failure, which varied between 1.89 and 3.92 in beams Nos. 1, 2, 8, 9, and 12.

The explanation of those discrepancies seemed to be the fact that the Code of Practice formula, which was based on permissible stresses in steel and concrete, resulted in higher working moments if the concrete strength were higher and compression steel were added ; in reality, however, even a weaker concrete than mix (a) was sufficient to develop the full strength of the tensile steel in beams of the type used in those experiments, provided that no earlier failure occurred owing to such factors as slip and diagonal tension. An increase of the moment at failure could not be obtained by increasing the concrete strength, but only by preventing failure in slip or in diagonal tension. The better result in beam No. 11 and the large reserve in beam No. 6 were due to the angles on top of the joist.

There were only two beams in which failure of concrete in compression was recorded as a partial cause of failure. It seemed, however, that, even in those two beams, the failure of concrete in compression was only secondary and was caused by excessive deformation of the steel. That was pointed out in the case of beam No. 13, whereas in beam No. 15 the yield of the horizontal bars was the primary cause of failure. Pure failure in compression could only be observed in over-reinforced beams in which the tensile stress of the steel at failure was below the yield-point. None of the beams had failed in that way, but only failure of that type would have justified the assumption that the working moments depended on the strength of the concrete. The cracks in beam No. 15 as shown in Figs. 12, Plate 1 (facing p. 114 §), proved that they were due to diagonal tension. With a high-grade concrete, such as had been used in beam No. 15, a much greater amount of steel than there was in that beam could be utilized to its yield-point, if provision were made for preventing failure in diagonal tension. That was proved by the tests on beam

No. 13, in which the bending moment at failure was 22.5 per cent. higher than for beam No. 15.

The Authors stated on p. 64§ that all specimens of that series were proportioned so that the calculated maximum tensile stresses in the steel, for a given load, were approximately the same. Consequently the load-factors, also, ought to have been approximately the same. They varied, however, between 1.72 and 4.79 (neglecting beam No. 10, in which the joist was split into two separate parts). The method suggested in the Code of Practice of calculating such beams did not, therefore, seem to be satisfactory.

Dr. Hajnal-Kónyi thought that much better agreement between working moments and moments at failure could be obtained if a method, based on the plastic theory, were developed for composite beams similar to the method suggested by Professor Saliger for reinforced-concrete beams*.

Such a method could have been established by further tests in which the yield-point of the steel in the joists and the ultimate strain of the concrete in prisms and in beams was determined. It was also essential to make tests in weak concrete so as to allow failure in compression only. The utilization of the full strength of the steel depended mainly on preventing failure in slip or in diagonal tension. Tests for investigating methods of attaining that purpose in the most suitable way were wanted. If failure in diagonal tension or slip could be avoided and the yield-point of the steel reached at failure, as in the case of properly designed reinforced-concrete beams, the application of composite beams in practice would be very advantageous.

Dr. A. D. Ross was interested in the increasing deflexions under sustained loadings recorded in the Paper. He did not consider it fortuitous that the ratio of the total deflexion (Δ_t) of various beams, at 150 days after loading to the initial elastic deflexion (Δ_e), should be between the relatively narrow limits of 2.40 and 3.05. Increase in deflexion was caused by creep of the concrete, by shrinkage, and by tension-cracking, but probably the bulk of the time-deflexion was due to creep. The amount of creep was governed by a number of factors, but, in general, a concrete with a high elastic modulus would evidence small creep and, conversely, a low-modulus concrete would show large creep values. Whilst no exact relationship was known to exist between elastic and creep deformations, the ratio between them at any particular age would not vary between very wide limits for any one concrete. Therefore, a beam which showed a large elastic deflexion would also show a large time or creep deflexion.

§ *Ibid.*

* Dr. K. Hajnal-Kónyi, "The Modular Ratio—III." *Concrete and Constructional Engineering*, vol. 32 (1937), p. 189 (March 1937).

[Professor R. Saliger's discussion on Dr. Hajnal-Kónyi's Paper appears on p. 293 of the same volume.—Sec. Inst. C.E.]

and Δ_t/Δ_e would vary within narrow limits only. He had suggested * that the creep of any concrete could be expressed as $\frac{t}{(100 + t)b} 10^{-6}$ inch per inch, t denoting the time in days from loading and b a coefficient depending on the stress, water/cement ratio, humidity, age at loading, and size. Dr. Faber's work was referred to in the Paper and, for the concrete of his beams, assuming a mean relative humidity of 65 per cent. and taking his 5-inch by 2-inch section as roughly equivalent to a 3-inch diameter cylinder, the estimated creep under unit stress at 150 days was $\frac{150}{(100 + 150)1.25} 10^{-6}$ inch per inch. That corresponded to an effective modulus of 1.37×10^6 lb. per square inch, which, if used for the calculation of deflexion, gave a value of 2.9 for Δ_t/Δ_e at 150 days. The 28-day strength and elastic modulus of the concrete employed in beams Nos. T₁ to T₈ appeared to be low for concrete having a water/cement ratio of 0.74 and made with rapid-hardening Portland cement. The figures were, in fact, very near to those given by Faber for his normal Portland-cement concrete with a water/cement ratio of about 0.78. Creep values of the same order of magnitude would therefore be expected, somewhat reduced, owing to the larger section of the beams Nos. T₁ to T₈. Since more than half the total shrinkage usually occurred in the first 28 days, the increase in deflexion of beams, due to shrinkage, from 28 to 150 days was probably small, but was of relatively greater significance in lightly loaded beams and caused an increase in the ratio $\Delta\Delta_t/\epsilon_s$, as recorded in the Paper.

He considered that the agreement indicated (*Figs. 29*, p. 101 §) between the calculated and measured steel shrinkage stresses was fortuitous. The values of free shrinkage were taken from 4-inch by 4-inch by 32-inch prisms, and those could not reasonably be assumed to represent the shrinkage of the beams, since shape and size enormously affected the movement. He had found large differences in the shrinkages of specimens of the same mix, stored under the same atmospheric conditions, but varying in size and shape. For example, the average shrinkage (measured on three gauge-lines) of a 1.5-inch by 1.5-inch by 9-inch prism was 96 per cent. more than that of a 6-inch by 3-inch by 9-inch prism at 34 days. The values of E_c and m employed in those calculations were not stated, but it seemed clear that creep would influence the result greatly. Presumably *Figs. 29* (p. 101 §) represented change in steel stress, since the bottom of the joist would already be in tension because of the dead load. The analysis of the stresses in a beam in such a condition was complex. In the concrete above the neutral axis, shrinkage would cause tensile stress due to the restraint of the steel, dead load would cause compressive stress,

* Dr. A. D. Ross, "Concrete Creep Data." *Journal I. Struct. E.*, vol. 15 (1937), p. 314 (August 1937).
§ *Ibid.*

and the resultant stress, whether tensile or compressive, would cause creep, which would be considerable since the concrete was green.

Most valuable results were presented in the Paper and the volume of experimental work had been very considerable. He did feel, however, that creep tests under axial stress would have yielded most useful information in giving a positive value for the effective modulus for that concrete, thus enabling the relationships between steel stress, deflection, and effective modulus to be investigated directly.

Professor Batho, replying for the Authors, observed that the experiments on beams Nos. 1, 2, 8, 9, and 12 showed, as stated in the Paper (p. 80 §) and also by Dr. Hajnal-Kónyi, the importance of the limitation of bond stresses or the provision of anchorages in order to prevent failure by slip or diagonal tension, at any rate until an adequate load-factor was reached. Provisions for the same purpose were required by the Code of Practice for Reinforced Concrete, and, therefore, beams Nos. 8, 9, and 12 could not be regarded as complying with the Code. He agreed, however, that the use of the Code of Practice was not the most economical method of designing composite beams. In certain cases it would even give a working load for the encased beam which was less than that for the bare joist. Application of the plastic theory, as suggested by Dr. Hajnal-Kónyi, would probably give more reasonable results, but even more economy might be obtained by taking account of the slow development of yield in the joist, which gave a reserve of strength above yield not obtainable in an ordinary reinforced-concrete beam (p. 77 §).

The method for the determination of the amount of creep given by Dr. Ross was interesting, and, when applied to determine the increase in deflexion with time, gave results in approximate agreement with those obtained from the beams tested in the laboratory under normal conditions. It could not, however, be applied to beams, such as those tested out of doors, in which considerable changes took place in the manner of interaction of the steel and the concrete.

Although the comparison between the actual and theoretical stresses in the lower flange of the joist due to shrinkage of the concrete (*Figs. 29*, p. 101 §) could only be a comparison of the "general trend" (p. 102 §) because of the complicated factors involved, it did not follow that the general agreement found was fortuitous. The effect of the variation of shrinkage across the section of the beam was clearly shown, and that had been confirmed in the course of further experiments now in progress.

He agreed that concrete tests under axial loading would have been interesting, but he doubted if the information derived from them would have helped materially in the investigations contained in the Paper.

Paper No. 5161.

"The Resistance to Flow of Water along a Tortuous Stretch of River and in a Scale Model of the Same."†

By JACK ALLEN, M.Sc., Assoc. M. Inst. C.E.

Correspondence.

Dr. Herbert Chatley observed that the Paper was an important contribution to hydraulic science in so far as it helped to distinguish bed friction from the eddy losses due to curvature and change of section.

In the discussion on Sir Robert Gales' Paper *, and in an earlier article in *Engineering* ‡, Dr. Chatley had suggested that there was a tendency in alluvial rivers for curvature and variations of section to develop a resistance equal to the bed friction. The Author's table of percentages (p. 120 §) indicated that the textural roughness resistance (bed friction) corresponded to about half the total head loss for the larger flows of the Mersey, although it was less for smaller flows. Prof. A. H. Gibson had indicated || that the bed friction might be much less than the tortuosity resistance, but Dr. Chatley was inclined to the view that that was only true for relatively rough channels in which the effect of rugosity was added to the effect of tortuosity. Small flows came within that category and conformed to the Author's results for low discharge of the Mersey.

If it were assumed that the head lost by tortuosity was equal to that lost by bed friction, half of the total head lost (after making any necessary adjustment for the change of kinetic head at the lower end) provided a means of deducing frictional coefficients for large and tortuous rivers, and so of controlling discharge formulas for very large values of the Reynolds number.

A very interesting point was the loss at each definite change of direction, perhaps two per complete bend. In the Mersey stretch, according to the Author, there were twenty-eight "major bends" and the total head lost,

† Journal Inst. C.E., vol. 11 (1938-39), p. 115 (February 1939).

* "The Principles of River-Training for Railway Bridges, and their Application to the Case of the Hardinge Bridge over the Lower Ganges at Sara." Journal Inst. C.E., vol. 10 (1938-39), p. 207 (December 1938).

‡ Dr. Herbert Chatley, "Frictional and Dissipational Fall in Rivers." *Engineering*, vol. cxlvi (1938), p. 62.

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—Sec. INST. C.E.

|| Vernon-Harcourt Lecture. "Tidal and River Models." Journal Inst. C.E., vol. 3 (1935-36), p. 709 (October 1936, supplement).

when the discharge was 7,000 cusecs, was 29 feet. Half of that was $14\frac{1}{2}$ feet, so that, on the assumption just described, the loss of head per bend due to tortuosity was of the order of $\frac{1}{2}$ foot. The mean velocity was about 5 feet per second, corresponding to a mean kinetic energy of 0.39 foot, so that the loss of head per bend was about 130 per cent., or 65 per cent. per change of direction if two per bend were assumed. At a curve with the usually observed distribution of the velocity in the cross section, a large fraction of the flow in the outer side of the curve was "irrotational", "free vortex" flow, decreasing with the distance from the centre of curvature. On the inner side of the curve, on the other hand, there was "rotational", "forced vortex" or quasi-solid flow, increasing with the distance from the centre of curvature. Whereas the former involved no internal spinning of the water and therefore no loss of kinetic energy by the formation of eddies, the latter was converted into a whirl which was irreversible. Consequently at a long turn where such conditions were fully developed there was a tendency for about half of the kinetic energy to be converted into irrecoverable spin, and the velocity of the water could only be maintained by a fall equal to that loss of head. In other words, a turn of appreciable length would probably cause a loss of head equal to about half the kinetic head.

Another aspect of the matter was the degree of tortuosity of a river. The thalweg length of the stretch of the Mersey under consideration was 2.25 times the air-line. It would be very interesting to have that figure recorded for all rivers, both for the whole alluvial length and also along the river according to the distance from the head of the plain, as a standard feature of river data. In those cases where the river was held at certain non-collinear points by rock masses, the "air-line" should be taken as a series of tangents between such points. In Sir Robert Gales' Paper * the bend/cut-off ratio at the Bell bunds was given as 1.75. That was not the same as the thalweg/air-line ratio, but it was related thereto. With a given breadth of stream there were geometrical limiting values for the thalweg/air-line ratio due to the back contact of bend with bend, but those values would be appreciably different for spiral curves from those found for circular curves.

If instability due to cut-offs were imminent when the thalweg/air-line ratio was, say, 2.0, and also the bed friction head was half the whole head, then the ideal straight channel would have a hydraulic friction gradient 4 times that in the actual tortuous river and would develop about twice the velocity which actually occurred in the tortuous channel. Such a condition in alluvium was essentially unstable, the excess velocity causing erosion and deepening. The latter would again cause increase of the velocity, until the disturbances caused by the large eroded masses initiated

* "The Principles of River-Training for Railway Bridges, and their Application to the Case of the Hardinge Bridge over the Lower Ganges at Sara." Journal Inst. C.E., vol. 10 (1938-39), p. 143 (December 1938).

bank erosion and curvature, with consequent loss of kinetic head and increased bed area.

Dr. Chatley referred to the Author's treatment of the effect of distortion on the effective hydraulic radius. He (Dr. Chatley) had shown † that, if the discharge (with constant frictional coefficient and constant slope) were simply an exponential function of the area and the hydraulic radius, there would be certain cases in which increase of the area at a much smaller rate than the wetted perimeter would cause decreased discharge. That was very improbable and suggested that a form factor additional to the hydraulic radius should be employed. That form factor would play a large part in model-experiments, and the case where, by distortion, a depression was reduced to a deep slot was undoubtedly a good example. A doubt was cast upon the results of all gaugings made on sections of an irregular form.

Mr. James Williamson thought that the outstanding feature of the river records, which, however, was not emphasized in the Paper, was the very large increase of fall with increase of flow. That would, at once, attract the attention of a hydraulic engineer as being abnormal to any regular water conduit, or to any natural stretch of river which would be considered suitable for the application of either Kutter's or Manning's formulas for flow. The effect of increasing the flow in any such channel was to increase the depth of flow in the whole stretch, and there was, therefore, no material change of gradient or increase of fall. In a river section of $7\frac{1}{2}$ miles considered by the Author, the fall was found to increase from 20 feet, for a flow of 350 cusecs, to 29 feet, for a flow of 7,000 cusecs. That extraordinary increase of fall would indicate the existence of some exceptional or unnatural conditions. The exceptional conditions which produced departure from normal river flow were to be found in the lower 2 miles of the section. Two bridges produced constrictions, but the major effect was produced by the Irlam weir at the lower end, over which the river passed into the Manchester Ship Canal. The weir formed a dam, its crest being at a considerable height above the former river bed, and served to pond the river for a considerable distance upstream and to affect the flow for about 2 miles. The weir was long in comparison with the average width of the river, so that much water could be passed over by a comparatively small rise in the water-level above the weir. The rise in level near the weir for an increase of flow of from 350 to 7,000 cusecs was found from Table VII, p. 125 §, to be 3.7 feet, which was small compared with the range of rise in the natural river channel. When the flow was small, ponding and the consequent low velocity resulted in the gradient above the weir being very small and very much less than the gradient of the normal river higher up. That condition rapidly altered with increase of flow, and at the higher flows steeper gradients were found in the lower

† Footnote (§), p. 387.

§ *Ibid.*

2 miles than elsewhere. The effect of the weir in producing varying gradients terminated near Flixton bridge, about 2 miles above the weir. The difference in the gradient conditions below and above Flixton bridge was shown by Table XIV, which indicated the variation of the fall in the two sections with alteration of the flow. The figures were deduced from Table VII, p. 125 §.

TABLE XIV.

Flow: cusecs.	Fall: feet.	
	Lower section: Irlam to Flixton (about 2 miles).	Upper section: Flixton to station 155 (about 6½ miles).
350	0·9	20·8
2,550	6·8	20·2
4,300	8·5	20·8
7,000	10·5	21·2

The difference between the figures for the two sections was seen to be very pronounced. In the lower section the average fall varied from less than $\frac{1}{2}$ foot per mile up to more than 5 feet per mile, whereas in the upper section the average fall was nearly constant at about 3·2 feet per mile, thus indicating normal river flow. The abnormal conditions in the lower section were produced by the weir, and that section represented the transition length required to connect a small rise of water-level at the weir with a large rise of water-level in the normal river higher up. In view of the artificial and abnormal conditions, the lower section might with advantage have been left out in the investigation of tortuous river flow. It appeared to Mr. Williamson that if the Author had confined his calculations and comparisons to the upper section, which did represent river conditions, his results would have been entirely different, and might have been more generally useful.

Referring to the Author's summary of conclusions (a) to (d) (p. 131 §), Mr. Williamson observed that:

(a) The Author's analysis of resistance was based on averaging, over a length of about 8 miles, a large increase of fall which was produced by artificial conditions in the lower 2 miles of the length. The results therefore did not apply to the conditions in a naturally tortuous river as represented by the upper 6 miles, where the fall was nearly constant.

(b) The Author's formula was empirical for a combination of an artificial and a natural stretch of river, and appeared to have no particular significance. It seemed an exaggeration to call the model, as finally used, a scale model, except in respect of the alignment of the water-course.

(c) The methods adopted by the Author for distorting a model, which did not at first reproduce the water-levels of the river, were not the only

methods that could have been adopted. By any method that could be visualized, the result would be lack of similarity in the form and no approach to similarity in the flow, other than in respect of water-level. It might have been that it was sufficient for the Author's purpose to reproduce water-levels only, but the practical purpose of the model thereafter was not clear.

(d) In a natural river, such as that under consideration, the conditions were beyond the range where viscosity had any material effect. In a small model, however, such as that used by the Author, viscosity had the effect of modifying the resistance. Heating the water would have lessened the viscosity coefficient and lowered the resistance. In any normal channel the result of lowering the viscosity coefficient would be to produce lesser depth of water and greater velocity, but no increase or decrease of fall.

The Author, in reply, pointed out that one object of the Paper was to provide quantitative evidence of the relative losses of energy due to (a) curvature and change of section, and (b) textural roughness, in a particular river. It had not been the Author's view that the actual proportions found for that river would necessarily apply to others of different roughness and tortuosity, or even to different sections of the same river. The general order of the results obtained certainly agreed, however, with Dr. Chatley's theory that "there was a tendency in alluvial rivers for curvature and variations of section to develop a resistance equal to the bed friction." Mr. Williamson, on the other hand, appeared to believe that the presence of the Irlam weir effectively reduced the value of the investigation. Granted that the water-level at station B was essentially controlled by the weir, the question arose how far the rise in level from B to A was due to bed-roughness and how far to the bends and changes of section; the information on that point was presented in the Paper and was meant to apply over that portion of the river. It was of interest, however, to make similar calculations for another portion of the river, and the Author had done so for that between station 155 and Flixton bridge, as suggested by Mr. Williamson. For that purpose he had used Bazin's formula, with the results set out in Table XV:

TABLE XV.—PERCENTAGE OF LOSS DUE TO TEXTURAL ROUGHNESS.

Q: cusecs.	Class B, $N = 2.35$.		Class C, $N = 3.17$.		Average of Classes B and C.	
	A to B.	155 to Flixton.	A to B.	155 to Flixton.	A to B.	155 to Flixton.
350	21	39	31	57	26	48
2,550	38	34	52	46	45	40
7,000	41	41	53	54	47	47.5

Those results were of interest as showing that, except with a very small

discharge, the values obtained for the stretch between station 155 and Flixton were almost the same as those previously given for the reach AB. The reason for that was to be traced to the complex nature of the cross-sectional shape of the river, which, whilst producing similar averaged velocities and hydraulic mean depths for moderate and high flows over the portion AB to those between Flixton and station 155, gave a much higher velocity and smaller value of m for the Flixton to 155 stretch than for the other reach at a very small flow.

The Author had also calculated the percentage loss due to textural roughness over the portion from station 155 to station 10, with the following results :

{	Bazin's $N = 3.17$,	percentage = 53	for $Q = 350$	cusecs
	„ $N = 2.35$,	„ = 37	„ $Q =$ „ „	
{	Bazin's $N = 3.17$,	„ = 51	„ $Q = 2,550$	„
	„ $N = 2.35$,	„ = 38	„ $Q =$ „ „	
{	Bazin's $N = 3.17$,	„ = 57	„ $Q = 7,000$	„
	„ $N = 2.35$,	„ = 44	„ $Q =$ „ „	

Regarding the question of how far the weir at Irlam influenced the river, it might be of interest to give details of experiments made by the Author on the model. In those tests, the weir-length was artificially restricted, thus raising the level immediately behind the weir by an amount equivalent to x_1 feet. The measured rise of level, x_2 feet, at station 5 (approximately $1\frac{1}{2}$ mile above the weir) was :

$$\begin{aligned}
 Q = 8,020 \text{ cusecs.} & \begin{cases} x_1 = 1.0; & x_2 = 0.0. \\ x_1 = 2.6; & x_2 = 0.1. \\ x_1 = 4.6; & x_2 = 0.2. \end{cases} \\
 Q = 4,030 \text{ cusecs.} & \begin{cases} x_1 = 1.4; & x_2 = 0.1. \\ x_1 = 3.6; & x_2 = 0.4. \\ x_1 = 6.0; & x_2 = 1.3. \end{cases} \\
 Q = 2,030 \text{ cusecs.} & \begin{cases} x_1 = 0.8; & x_2 = 0.0. \\ x_1 = 2.7; & x_2 = 0.6. \\ x_1 = 4.7; & x_2 = 1.8. \end{cases}
 \end{aligned}$$

The backwater effect even at station 5 was thus seen to be relatively very small over that range of discharges.

Contrary to Mr. Williamson's view, the Author was of opinion that the way in which the formula $v = Km^{\frac{1}{2}}x^{\frac{1}{2}}$ represented the state of affairs in both model and river with the same values of K and x was extremely remarkable, and sufficiently significant to suggest that an equation of that type might be found valuable in analysing the behaviour of other rivers.

As to the practical purpose of the model, it was explained in the Paper that it was the investigation of problems of cut-offs; space would clearly not permit details of that work also to be included.

Concerning the effect of viscosity, the level at the outfall of a river was determined by conditions there: in the present case by the weir. Unless the coefficient of the weir was affected by temperature, the level there would remain constant for a given discharge. If, then, a lowering

of the viscosity-coefficient produced a lesser depth of water, surely that could only be accompanied by a decrease of fall? The significance of the model-results was that the change in fall, resulting from a very considerable alteration of viscosity, was extremely small, despite the low Reynolds numbers involved. An explanation of that phenomenon was offered in the Paper.

Paper No. 5204.

"Turbulent Flow in Pipes, with particular reference to the Transition Region between the Smooth and Rough Pipe Laws."†

By CYRIL FRANK COLEBROOK, Ph.D., B.Sc. (Eng.), Assoc. M. Inst. C.E.

Correspondence.

Mr. Thomas Blench suggested that the Author might be interested in his treatment of exponential formulas* which showed that such a formula could cover all cases. A more logical transition formula might be derived therefrom. In fact, the formula

$$V = C \left(\frac{m}{x} \right)^{\frac{1}{4}} \sqrt{gmS} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where C was an absolute constant, m denoted the hydraulic mean depth, S was the non-dimensional pressure gradient, and x was a linear measure of roughness, reduced to special cases by suitable choice of x .

If the boundary were smooth, x denoted the laminar film thickness δ , and Prandtl's deduction that relative laminar film thickness varied inversely as \sqrt{R} , gave

$$V = C_s \left(\frac{Vm}{\nu} \right)^{\frac{1}{4}} \sqrt{gmS}$$

which was best known in the universally accepted form derived by Blasius:

$$V = \text{constant } m^{\frac{1}{4}} S^{\frac{1}{4}}$$

† Journal Inst. C.E., vol. 11 (1938-39), p. 133 (February 1939).

* T. Blench, "A New Theory of Turbulent Flow in Liquids of Small Viscosity." Journal Inst. C.E., vol. 11 (1938-39), p. 611 (April 1939).

If the boundary were rough, x was the factor which the Author denoted by k .
Then

$$V = C \left(\frac{m}{k} \right)^{\frac{1}{4}} \sqrt{gmS},$$

or

$$V = \text{constant} \times R^{\frac{3}{4}} S^{\frac{1}{4}},$$

which was very like Manning's formula.

If the boundary were "incoherent", as in channels which had formed themselves finally in their own transported material, x was a certain number (containing $(\nu g)^{\frac{1}{3}}$) times f , the Lacey silt-factor, and Lacey's flow equation † was the result :

$$V = \text{constant} \left(\frac{m}{f} \right)^{\frac{1}{4}} \sqrt{gmS}$$

Mr. Blench thought that at that point it might be mentioned that von Kármán's u_* was \sqrt{gmS} .

Accepting the truth of formula (1), it was reasonable to think that the rate of working per lb. of fluid, under transition conditions, would be expressible partly in terms of k and partly in terms of δ . That rate of working was

$$gVS = \frac{V^2}{C^2 m} \left(\frac{x}{m} \right)^{\frac{1}{2}} \dots \dots \dots (2).$$

In the transition zone, the result might be expected to be

$$gVS = \frac{V^3}{C^2 m} \left[a \left(\frac{k}{m} \right)^{\frac{1}{2}} + b \left(\frac{\nu}{Vm} \right)^{\frac{1}{2}} \right] \dots \dots \dots (3).$$

Since the thinning of the laminar film with increase of R caused more and more roughnesses to penetrate that film, a and b would be functions, probably simple powers, of R . The foregoing led, finally, to a suggested logical formula for test :

$$gVS = \left(\frac{V^3}{C^2 m} \right) \left[c \left(\frac{k}{m} \right)^{\frac{1}{2}} R^p + d R^q \right].$$

Such a formula, if verified, would have a dynamical basis which was missing from any formula derived from von Kármán's theory.

Mr. Blench pointed out that the Author's equation (1) (p. 135 §) was derived from Prandtl's approximation to von Kármán's original velocity formula. He thought it necessary to emphasize that, in spite of the tre-

† G. Lacey, "Uniform Flow in Alluvial Rivers and Canals." Minutes of Proceedings Inst. C.E., vol. 237 (1933-34, Part I), p. 421.

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 11 (1938-39), p. 133 (February 1939)).—SEC. INST. C.E.

enormous advance given by the von Kármán theory, the resulting equations were not final. He indicated that research might be directed on wrong lines by a too-ready acceptance of equations such as (1), (2), and (3) (p.135§).

Fig. 10.

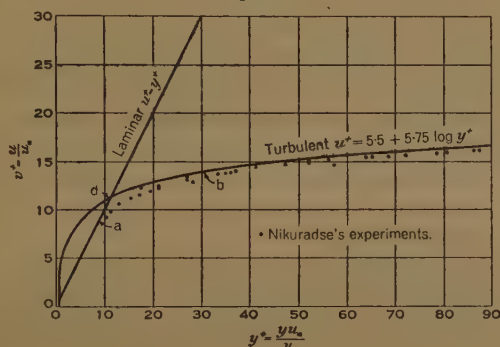
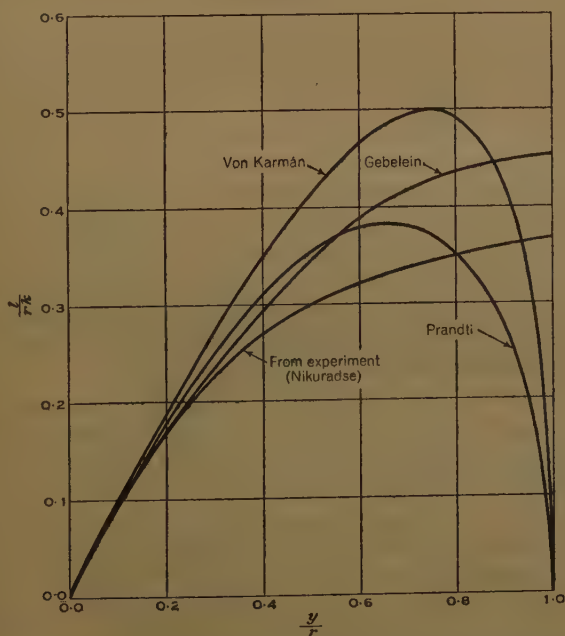


Fig. 11.



The defects of the theory were discussed, with full appreciation of the theory itself, by Bakhmeteff*. Figs. 10 and 11 were reproduced from

§ *Ibid.*

* B. A. Bakhmeteff, "The Mechanics of Turbulent Flow", pp. 61-98. Princeton, 1936.

Figs. 53 (p. 82 §§) and *49* (p. 73 §§), respectively. *Fig. 10* showed how the theoretical universal velocity distribution curve (Prandtl's modification) in appropriate variables, gave a cusp at the pipe centre, and deviated from data up to the point where y^+ was approximately equal to 25. *Fig. 11* showed how mixing length, as actually found, differed from theory.

λ was found by integrating the expression for velocity, from Prandtl's amended form of von Kármán's equation, between δ and r , and dividing by the pipe area to obtain the mean velocity V . The lower limit had to be chosen to avoid the difficulty that $\log 0$ was infinite. As the value obtained from V contained u_* , which was proportional to \sqrt{gmS} , a flow equation resulted. Exactly the same form of argument would derive a flow equation of equally good fit to data if a parabolic or other type of curve were used to replace the logarithmic type.

The advantage of using the exponential flow formula was its dynamical derivation and its freedom from the theory of velocity distribution. Results had, therefore, some likelihood of sharing its dynamical soundness and indicating the physical principles that lay behind them. Von Kármán's formulas were not dynamically perfect, although his choice of new variables was sound.

Dr. Herbert Chatley suggested that the theories expounded in the Paper would be rather obscure unless studied in conjunction with the Paper * by Drs. Colebrook and White.

Referring to the Author's treatment (p. 134 §) of the Reynolds type of formula, which formed the basis of many exponential rules, it would be expedient to show the manner in which the reputed relation between the two indices was derived. Dr. Chatley had treated it in his Paper on "River Discharge Formulæ" delivered to the Engineering Society of China in 1930, but it was not often mentioned in engineering texts. The fact that the relation was not true when roughness was allowed for was very important, but its truth for smooth surfaces was often overlooked and serious errors in discharge formulas were allowed to go uncorrected.

The treatment of the Prandtl-von Kármán theory (pp. 134, 135 §§) might with advantage have reproduced some of the matter in the earlier Paper *, as it was by no means clear that y was a distance from the wall of the pipe at which mixing of the water was assumed to be the same as at the wall itself. Dr. Chatley thought that when the expression "lower limit of integration" was introduced, it should have been explained that the velocity-distribution curve was being used.

The subject of "shear force" at the walls might have been expanded

§§ Page Numbers so marked refer to Bakhmeteff's book. (Footnote (*), p. 395.)
—SEC. INST. C.E.

* C. F. Colebrook and C. M. White, "The Reduction of Carrying Capacity of Pipes with Age." *Journal Inst. C.E.*, vol. 7 (1937-38), p. 99 (November 1937).

§ *Ibid.*

with some advantage, as that was the fundamental source of the turbulence. It was closely related to Du Boys' "tractive force."

Dr. Chatley admired the ingenious manner in which the mathematical difficulties were evaded by the use of special co-ordinates in the figures, but thought that the method would be much more illuminating if the Author would give one or two conversions to direct relations of the resistance coefficient to the roughness Reynolds number. What might have been still better would be a curve showing the relation of the resistance coefficient to the relative roughness for constant Reynolds number.

He did not see the reason for the assertion (pp. 139, 140 §) that formula (10) so definitely led to formula (11), since there was the possibility of terms involving powers occurring.

The final Tables, using the de Chezy formula, with tabulated coefficients, simplified matters considerably and were quite in accordance with aerodynamical practice. They showed, however, a distinct departure from the formulas of Manning and Forchheimer, or even from the rules of Ganguillet and Kutter.

Mr. E. H. Essex, in making his observations, used the following notation :

C denoted the Chezy coefficient $\left(= \sqrt{\frac{2g}{f}} \right)$.

$$f \quad \text{,,} \quad \text{,,} \quad \text{gravitational coefficient of resistance} \left(= \frac{2F}{\rho V^2} = \frac{2g}{C^2} = \frac{\lambda}{4} \right).$$
$$F \quad \text{,,} \quad \text{,,} \quad \text{drag per unit area} \left(= \frac{Wh}{PV^2} = RSw = f\rho \frac{V^2}{2} \right).$$
$$\frac{F}{\overline{V_2}} \quad ,, \quad ,, \quad \text{unit drag} \left(= \frac{w}{C^2} = \frac{\rho f}{2} = \frac{\rho \lambda}{8} \right).$$

g " " acceleration due to gravity.

h " " loss of head per second.

H " " total loss of head.

PV " " wetted surface in unit time.

Q " " discharge.

R " " ratio area/perimeter, or Q/PV .

R " " ratio area/perimeter, or $\frac{A}{P}$ ft.

S " " ratio (total loss of head)/(length of pipe or channel)
 $= H/L$.

V " " mean velocity of flow.

w " " specific weight ($= \rho g$).

w^H " " pressure loss.

Wh " " work done per second ($= PRVwSV$).

W_h	"	"	work done per second
y_1	"	"	ideal thickness of the boundary layer of flow.

Mr. Essex observed that the Author had presented a new theoretical argument based upon the more recent laboratory experiments of Nikuradse

§ *Ibid.*

with sand-roughened surfaces in small-diameter tubes, but he did not state precisely what was found to be the resistance drag in foot-pound-second units. Froude gave the value of unit drag for fine sand as 0.008 lb. feet² per second and it compared with 0.003 for a smooth surface. If, however, the length of wetted surface were measured over the face of the sand grains instead of through them, an area of 1 square foot would give about $1.5 \times 1.5 = 2.25$ square feet of wetted surface; whence Froude's value, 0.008, became 0.0035, which more nearly resembled the value of unit drag on the smooth surface, and seemed to indicate an arithmetical increase of wetted surface area rather than any increase on impact losses in

or near the boundary layer. Brass tubes had a uniform $\frac{C}{\sqrt{w}}$ value of

$3.42 \log \frac{\rho}{\mu} RV$, which gave a value of $27 \log \frac{\rho}{\mu} RV$ for C . For water at

59° F., $\log \frac{\rho}{\mu}$ was 4.91, so that when $RV = 1$, $C = 132.7$ and the unit drag

was $\frac{w}{C^2} = 0.0035$ lb. feet² per second. The $\frac{3}{8}$ -inch to $\frac{7}{8}$ -inch pebbles in

cement of D'Arcy's series 4 showed a fairly regular scale-factor of $A = 14.5$,

and C would be 71, giving a unit drag value of $\frac{w}{C^2} = 0.0123$ lb. feet² per

second. If correction were applied for true wetted surface area, unit

drag became $\frac{0.0123}{2.25} = 0.0055$; the excess drag was probably due to

impact losses outside the boundary layer. Channel design would, however,

still be based on the value of $\frac{C}{\log \frac{\rho}{\mu} RV} = 14.5$, because it was known that

the value of C derived in that way corrected the inaccuracy of the observed drag; that was to say, it was the observed overall drag that was desired for the purpose of design. It was desirable to limit the drag in a loose channel to the flat area of the sand grains, because, by so doing, allowance was made for a margin of safety against silt carriage. Practically, it was of minor importance which value of drag was used if the ratio P/R were known. That ratio, however, although of equal physical importance to the ratio $\frac{V}{\sqrt{S}}$, was usually hidden in the formula and so was not taken into

full consideration. If P/R were plotted on the ordinate of $RV = 1$ to the

parameter $\frac{\rho}{\mu} RV$, a line drawn at 45 degrees would give values of PV for all

values of RV . The ratio P/R was non-dimensional, but there were many

dimensional factors of great physical importance, such as the ratio $\frac{V}{\sqrt{S}}$

ferred to. It was questionable to what extent Nikuradse had considered the ratio of the practically undiminished sectional area to the greater wetted surface area in his sand-covered pipes, and how he had calculated his values of R and S . The correct value of the ratio area/perimeter would be of the nature of $d/6$, and the ratio P/R would be 6π , or 18.85, instead of the 12.566 commonly adopted. That became of greater importance when consideration was given to the drag in lb. feet² per second, to be obtained from $PRVwh$, as required for any attempt at close comparison of the result of pipe flows with the flow in channels; it seemed clear that the only rational scale-factor was the rugosity value of

$$A = \frac{C}{\log \frac{\rho}{\mu} RV}$$

Full particulars of the 4-inch-diameter galvanized-iron pipes, upon which the Author had based the major portion of his transition curves, had been published*. For $V = 1.059$, C was 95.39, and with $\log \frac{\rho}{\mu} RV = 3.80$ (temperature being 51.5° F.), A was 25; that, however, was clearly an error of observation, for the two readings on either side gave values of $A = 24$, which was in accordance with many other tubes of that material and could not be claimed as a standard of smoothness. With $A = 24$ and $\log \frac{\rho}{\mu} RV = 3.77$, C was 90.5 and the unit drag was $\frac{w}{C^2} = 0.007$.

For a reasonably smooth 4-inch-diameter pipe, having a value of $A = 27$, unit drag would be 0.006, C being 102; the ratio of smoothness, of the order of 0.006 to 0.007, was 1 : 1.16. Without first passing air, steam, or oil through the same tubes, it was speculative to suggest what proportion of that ratio was due to increase in wetted length or decrease in R . It was known that increase in perimeter would affect the unit drag, and that the decrease in R would affect the Reynolds number.

Mr. Essex stressed the importance of the rugosity scale-factor $A = \frac{C}{\log \frac{\rho}{\mu} RV}$ and pointed out that it was applicable to any single gauging,

adding the proviso that the velocity during any test should be carefully considered, for a high velocity had a tendency to reduce unduly the value of A .

The Stanton curve showed that the Chezy number was a physical function of the Reynolds number, and, when written as $28 \log \frac{\rho}{\mu} RV$, would represent the smoothest surface known. $E = 4.9 \log \frac{\rho}{\mu} RV$ gave

* F. Heywood, "The Flow of Water in Pipes and Channels." Minutes of Proceedings Inst. C.E., vol. ccxix (1924-25, Part 1), p. 174.

values of $\frac{C}{\sqrt{g}}$ which were non-dimensional. For $RV = 1$, $\log \frac{\rho}{\mu}$, for water at 59° F., was 4.91 and C would then be 137.5, whilst unit drag would be $\frac{w}{C^2} = 0.0033$ lb.-feet² per second. Mr. Essex compared that value with Froude's value of 0.003 for boards covered with tinfoil. The resistance throughout laminar flow continued in the boundary layer throughout turbulent flow, but total drag would increase with the cube of the velocity and would act mostly in the boundary layer. When flow velocities were excessive the drag in the boundary layer might take some of the boundary with it. The best formula for calculating the ideal thickness of the boundary layer, giving the numerical measure of the roughness of a pipe, might be written

$$\frac{2y_1}{d} = 1 - \left[1 - \frac{C^2}{g} \cdot \frac{2\mu}{\rho RV} \right]^{\frac{1}{4}};$$

that gave a boundary-layer thickness of $\frac{0.0018}{\text{diameter}}$ when substituting

$C = 137.5$, $A = 28$, and $RV = 1$. The Paper substantiated Mr. Essex's view that flow over rough surfaces would continue to follow the com-

pound-interest law for any value of $\frac{\rho}{\mu} R$ and also, with reasonable

velocities, any value of $\frac{\rho}{\mu} RV$. Some value of the rugosity factor would

be needed to give the correct value of C . From the Tables in the Appendix to the Paper, Mr. Essex had compiled the corresponding values of the

rugosity scale-factor A , taking $\log \frac{\rho}{\mu}$ as 4.91 with a velocity of 1 foot per

second and also a velocity of 3 feet per second. Those values were set out in Table VII (pp. 402, 403, *post*), and included also the corresponding values of Manning's number N for rugosity.

Values of $A = \frac{C}{\log \frac{\rho}{\mu} RV}$ had been found up to $V = 1$ foot and $V = 1$

metre. Beyond that point S varied as V^n and in certain pipes and for flat plates the friction increased not exactly as V^3 but as V^{n+1} ; n might be 1.75, 1.80, 1.90, or even 2.0, but only greater than 2.0 in the transition stage. The Author had, admittedly, only sketched his transition curves for the

Chezy C values from that point onwards to suit the values of $\frac{1}{\sqrt{\lambda}}$ recorded by Nikuradse; those recorded values, however, should be increased by at least one-eighth (on account of the increased wetted surface area in relation to

the cross-sectional area) before his frictional coefficient could be fairly compared with that which was found in smooth tubes. In such a case there was lost the meticulous accuracy which the Author seemed to claim for the Chezy numbers given in the four Tables in the Appendix. That consideration might help the Author to revise his views on what he called the square law, which law, Mr. Essex thought, was not substantiated by fact. The Author would find no sign of the square law in D'Arcy's channel, series No. 4, for $\frac{3}{8}$ -inch to $\frac{7}{8}$ -inch pebbles in cement; that channel surely had a rough enough surface to indicate such a law if it existed.

Mr. Essex had prepared a diagram (Fig. 12, facing p. 404) of flow values which should be of use in deciding at what point it might be deemed advisable, on account of uneconomically high velocities and for philosophical considerations only, to substitute a logarithmic curve for the compound-interest curve. As that diagram was plotted on the basis of $C=25 \log \frac{\rho}{\mu} RV$, the gradient curve represented a value of $V^{1.8}$ as shown by the full line, whilst V^2 was indicated by the broken line. He had indicated the method of construction of the diagram to show its comparison with the Author's theory, and had marked thereon the points for the examples given by the Author in the Appendix to the Paper. Example 1 presented no difficulty. The diameter was given as 48 inches and the gradient as 1 in 6,000. It would be noticed that the "key line" on the diagram contained all the diameter numbers and passed through the points where the discharge readings were the same as the readings for $\frac{1}{\sqrt{S}}$. For instance, referring to the example, the key line passed through the intersection of the discharge line corresponding to 48 inches diameter and the $\frac{1}{\sqrt{S}}$ line corresponding to 48 inches diameter. To solve the problem, the gradient line was followed to the abscissa corresponding to a gradient of 1 in 6,000 (point F). FG ($RV = 1.67$) was drawn vertically to cut the discharge line at G where the abscissa for discharge was 21 cusecs, the required solution. Example 2 was not so simple but still presented no real difficulty. It was required to find the diameter of a pipe of gradient 1 in 400 to give a discharge of 10 cusecs. It would be seen that the abscissa for gradient 1 in 400 cut the key line at the point F' which corresponded to a diameter of between 24 inches and 30 inches; the abscissa for 10 cusecs cut the key line at the point G' which corresponded to a diameter of between 15 inches and 18 inches. Precisely half-way between the points F' and G' the key line was cut by the discharge and $\frac{1}{\sqrt{S}}$ lines corresponding to 21 inches diameter which was the required diameter. Example 3 might also be examined on the same diagram. It was required to find the diameter of an asphalted cast-iron pipe which would discharge 36 cusecs

TABLE VII.—CLASSIFICATION OF THE CHEZY *C* VALUES FOR VELOCITIES
AUTHOR'S DRAG AND MANNING'S DRAG, AND ESSEX'S SCALE

Reference.	Diameter: inches.	Chezy " <i>C</i> "	Manning.	
			$1.486 \times \sqrt[n]{R}$	$\frac{1.486 \sqrt[n]{R}}{C} = "N"$
Smooth pipes	6	107	1.03	0.0096
		121		0.00985
	24	125	1.32	0.0106
		138.5		0.0095
	48	133.5	1.486	0.0111
New galvanized iron	6	147.5	1.61	0.0101
		141		0.0114
	24	145.5	0.775	0.0104
		75.5		0.0102
	4	83	0.980	0.0093
New asphalted cast iron	6	93.5	1.03	0.0105
		101		0.0097
	24	98.5	1.17	0.0104
		100		0.0103
	48	107.5	1.53	0.0117
New wrought-iron	6	115.5	1.03	0.0111
		103		0.010
	24	111	1.32	0.0103
		121.5		0.0108
	48	129.5	1.486	0.0102
New wrought-iron	6	130.5	1.53	0.0114
		139		0.0107
	24	133.5	0.775	0.0115
		141.5		0.0108
	48	141.5	0.980	0.0095
New wrought-iron	6	82	1.03	0.0095
		91.5		0.0085
	24	99.5	1.32	0.0098
		109		0.0090
	48	105	1.486	0.0098
New wrought-iron	6	114.5	1.61	0.0090
		113.5		0.0103
	24	123.5	0.775	0.0095
		82		0.0095
	48	91.5	0.980	0.0085

NOTE:—Roman figures apply to a velocity of 1 foot per second.

30 years after laying with a gradient of 1 in 100. The rugosity value on which the diagram was based was $A = 25$ and outside observations had shown that that value varied by an amount of from 0.4 to 0.5 per annum. Taking the latter figure, the value of A would fall, in 30 years, from 25 to 10 and the ultimate discharge of 36 cusecs would have to commence at 36×25

$\frac{10}{90} = 90$. It would be seen that the 90-cusec abscissa cut the key line at a point below that corresponding to 96 inches diameter whilst the 1-in-100-gradient abscissa cut the key line at a point between those corresponding to diameters of 15 inches and 18 inches; the nearest diameter to a point half-way between those two intersections was 36 inches. A 36-inch-diameter pipe would discharge 100 cusecs (at $RV = 10$) with a gradient of 1 in 100 whilst 30-inch and 33-inch-diameter pipes would

CITIES OF 1 AND 3 FEET PER SECOND FROM THE AUTHOR'S TABLES,

FACTOR "A" TAKING $\text{Log } \frac{\rho}{\mu}$ AT 4.91 FOR WATER AT 59° F.

Author's unit drag (unit drag = w/C^2): lb. per square foot.	Scale factor.	
	$\text{Log } \frac{\rho}{\mu} RV.$	$C/\text{Log } \frac{\rho}{\mu} RV = "A"$
0.0054	4.00	26.9
0.0043	4.48	27.0
0.0040	4.61	27.1
0.0032	5.09	27.2
0.0035	4.91	27.2
0.0029	5.39	27.4
0.0031	5.15	27.4
0.0026	5.63	27.5
0.0110	3.23	23.3
0.0091	3.71	22.4
0.0072	3.83	25
0.0061	4.31	23.4
0.0065	4.00	24.6
0.0062	4.48	23.3
0.0054	4.30	25
0.0049	4.78	24.1
0.0059	4.00	25.7
0.0051	4.48	24.8
0.0042	4.61	26.3
0.0038	5.09	25.5
0.0037	4.91	26.6
0.0032	5.39	25.8
0.0035	5.01	26.6
0.0031	5.49	25.7
0.0093	3.23	25.4
0.0075	3.71	24.7
0.0063	3.83	26
0.0053	4.31	25.2
0.0057	4.00	26.2
0.0048	4.48	25.5
0.0049	4.30	26.4
0.0041	4.78	25.8

Figures in italics apply to a velocity of 3 feet per second.

discharge 55 and 80 cusecs respectively. If the 36-inch-diameter pipe were adopted it would be capable of discharging 36 cusecs with a gradient of 1 in 700 and $RV = 3.7$. A new pipe to discharge 36 cusecs with a gradient of 1 in 100 would require to be 27 inches in diameter. It might be noted that to correct either Q or $\frac{1}{\sqrt{S}}$ for any value of A other than 25,

the reading from the diagram needed only to be divided by 25 and multiplied by the required value of A . Mr. Essex had plotted on the diagram the Author's amended figures for the 216-inch-diameter Ontario power conduit given in Table I (p. 145 §). He had also plotted the lines for Desmond Fitzgerald's 48-inch cast-iron pipes (after cleaning), Saph's and

Schoder's 1-inch and Heywood's 2-inch-diameter galvanized-iron pipes, together with the 6-inch-diameter cast-iron pumping main at Georgetown*. The last-mentioned pipe had a rugosity value of $A = 23$ but it was impossible to fit the Manning formula accurately. Any decision upon the question of the best value of V^n to be adopted would be better left to the practical engineer; in any event the line would lie between the full line for $V^{1.8}$ and the broken line for V^2 and the engineer could see precisely what he was doing. Mr. F. C. Scobey, referred to by the Author on pp. 143 § and 152 §, preferred the value $V^{1.9}$ which lay exactly half-way between the $V^{1.8}$ and V^2 lines shown in *Fig. 12*. Mr. Scobey had, himself, given rugosity values for all classes of steel riveted pipes, and his preference for the value of $V^{1.9}$ and $R^{1.1}$ made a comparison with the Chezy number a comparatively easy matter. On p. 134 § the Author suggested that Reynold's practical advice to make the sum of those two indices equal to 3 could only apply to smooth pipes, and that his own type of formula showed it to be inapplicable to rough pipes; clearly, however, the reason that the formulas of Williams and Hazen, and of Saph and Schoder, for cast-iron pipes had, in America, been proved the best formulas for comparative purposes was because, when converted to the RV parameter, they might, for $C_w = 140$, be written

$$C = 124 R^{0.0835} \cdot V^{0.074} \text{ and } C = 119 R^{0.1175} \cdot V^{0.075}$$

respectively. If those values, together with those of Barnes

$$(C = 86.7 R^{0.0055} \cdot V^{0.10} \text{ for glass and } C = 126 R^{0.102} \cdot V^{0.0615}$$

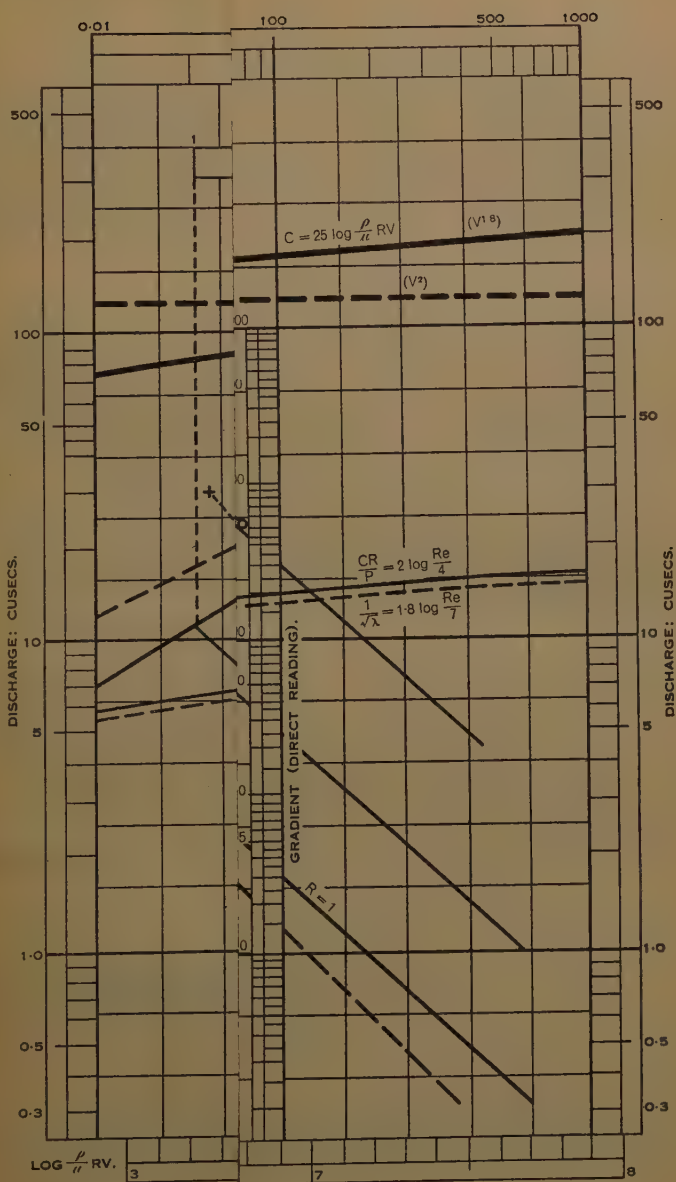
for smooth concrete) were plotted against the curve

$$C = 25 \log \frac{R_s}{4},$$

the practical importance of Reynolds's advice would be more fully appreciated. Mr. Essex thought that it would be safer to use any exponential formula, replotted to the RV parameter, rather than the Author's formula, which was based on an empirical curve and finished on the basis of Nikuradse's doubtful constants. It was generally recognized that, apart from losses due to impact, the resistance λ was confined to the laminar layer at the boundary. For that reason, Mr. Essex thought that the formula $\lambda = AR^n$, referred to on p. 134 §, was obsolete, although the thickness of the boundary layer was usually interpreted by that type of formula. It could be shown by dimensional theory that if y_1 denoted the thickness of the boundary layer and $\frac{y_1}{d} = A \left(\frac{\mu}{\rho V d} \right)^n$ where A and n were

* G. M. Humphreys and I. M. E. Aitken, "The Main Drainage of Georgetown British Guiana." Minutes of Proceedings Inst. C.E., vol. 236 (1932-33, Part 2), Table VI, p. 261.

§ *Ibid.*



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variable constants determined by comparing the formula with experimental data, that formula might be written

$$y_1 = A \left(\frac{\mu}{\rho V} \right) d^{1-n},$$

showing that if n were positive and less than unity then y_1 would be infinite when d (as in a flat plate) was infinite. That was absurd and merely afforded a further example of limitation in the dimensional theory. It was upon that assumption that Professor Lees based his formula for the Stanton curve, correcting the error by writing

$$\lambda = A \left(\frac{\mu}{\rho V d} \right)^n + B.$$

The addition of B was a mathematical manipulation for turning a straight logarithmic line into a hyperbolic curve and B had no claim to physical importance in spite of the attempt, made later*, to affirm that it represented the rugosity ratio. Mr. Essex offered the criticism that the theory of dimensions produced no constants of any practical significance since they had to be obtained from experimental data, as in the production of the Prandtl-von Kármán curve, which was claimed by the Author to represent the smooth law for fluid flow. It did not seem very long since a general study was made of von Kármán's suggestion to alter the indices of the Blasius formula for pipes from $\frac{1}{4}$ to $\frac{1}{5}$ for flat plates. Traces of that adjustment were still apparent in the curve. Since, however, Mr. Essex had evaluated the Stanton curve to the law of compound interest he had observed a growing tendency for philosophical formulas to follow suit, and the evaluation of the Prandtl-von Kármán curve had become $C = 28.9 \log \frac{R_e}{7}$. The Author admitted (p. 141 §) the practical impotence of his formula

$$\frac{1}{\sqrt{\lambda}} = 2 \log \frac{R_e 2\sqrt{\lambda}}{2.51}$$

in which the resistance value had to be known before it could be calculated, and recommended the use of a mathematical approximation,

$$\frac{1}{\sqrt{\lambda}} = 1.8 \log \frac{R_e}{7} \text{ or } \frac{C}{\sqrt{g}} = 5.1 \log \frac{R_e}{7},$$

which, he claimed, gave numerical results within $\pm \frac{1}{2}$ per cent. over a range of Reynolds numbers from 5,000 to 100,000,000. The Author

* F. Heywood, "The Flow of Water in Pipes and Channels." Minutes of Proceedings Inst. C.E., vol. cxxix (1924-25, Part 1), p. 174.

§ *Ibid.*

could have adopted outright Mr. Essex's value $\frac{CR}{P} = 2 \log \frac{R_e}{4}$ for commercial pipes.

Mr. Essex had a preference for writing his evaluation of the Stanton curve as $C = 27 \log \frac{\rho}{\mu} RV$ because RV was a physical function, the ordinate $RV = 1$ held all the constants of practical use to the engineer, and the ratio $\frac{d}{4}$ was the value usually used for pipes in philosophical laboratory records.

The value $\frac{R_e}{4}$ had, therefore, a physical importance for comparative purposes whilst Kármán's $\frac{R_e}{7}$ had nothing to recommend it, unless it claimed to have some philosophical value which was too complicated to define.

The arithmetic of the two curves was given in Table VIII and showed that the error of the Author's approximate von Kármán curve compared with the Stanton curve was ± 3 per cent.

TABLE VIII.

$\log \frac{R_e}{7}$	R_e	$\log \frac{R_e}{4}$	$1.8 \log \frac{R_e}{7}$	C_k	$\frac{C_k}{\log \frac{R_e}{7}}$	Stanton.		Percentage error in C_k	
						A	C_s	+	-
2.854	5,000	3.097	5.13	82.5	28.9	27	83.5	—	1.2
4.667	325,000	4.910	8.40	135	28.9	27	133	1.5	—
7.155	100,000,000	7.398	12.88	206	28.9	27	200	3.0	—

The physical function RV could be obtained in the field by dividing a measured quantity by a measured surface-area. That could not be done in the case of small tubes in the laboratory, so that a comparison was apt to be made wrongly between what might be called the engineer's practical C value and what might be termed the philosophical C value of laboratory records. German methods of tabulation left much to be desired in that respect; the figures of Nikuradse tended to confirm that a wrong conception of the physical ratio $\frac{P}{R}$ destroyed the value of most academic theories.

If the ratio $\frac{P}{R}$ did not vary with the depth of flow (as in the case of full-flowing pipes or V-notches) and was plotted on the unity ordinate of a logarithmic chart to the RV parameter, it represented PV , and a point representing $\rho \frac{P}{R}$ would give the constant through which a line drawn at $22\frac{1}{2}$ degrees would give readings for $\rho V^2 A$ for any value of RV . That constituted a check upon any type of flow formula.

Mr. Essex had plotted (*Fig. 13*, p. 408) the $\frac{C}{\sqrt{8g}}$ value of the three smooth-law curves referred to in the correspondence on the Paper by the Author and Dr. White *. The arithmetic shown in Table IX appeared to

TABLE IX.

Prandtl-von-Kármán.				Stanton.		Lees.	
$\log R_e \sqrt{\lambda}$	$\frac{1}{\sqrt{\lambda}}$	C	$\log \frac{R_e}{4}$	$E=1.68$	$A=27$	$\frac{1}{\sqrt{\lambda}}$	C
				$\frac{1}{\sqrt{\lambda}}$	C		
3	5.1	82	3.097	5.20	84	5.5	88
4	7.2	116	4.243	7.11	115	7.2	116
5	9.2	148	5.352	9.00	145	9.1	146
6	11.2	179	6.439	10.81	174	10.4	167

cover the whole argument. The Author recommended the Prandtl-von Kármán curve because it had been gradually worked out on philosophical lines (with many corrections to suit experimental data) from the internal motions which were the real cause of the resistance; he evaluated that curve at $C = 28.9 \log \frac{R_e}{7}$ which could be compared easily with Mr. Essex's

evaluation of the Stanton curve at $C = 27 \log \frac{R_e}{4}$. Mr. Essex preferred

to write that as $C = 27 \log \frac{\rho}{\mu} RV$, where RV was a measurable ratio of the discharge and a physical function of the Chezy number, the Froude number and the Reynolds number. He emphasized the suitability of RV as a parameter by pointing out that when multiplied by $4 \frac{\rho}{\mu}$ it represented

$\frac{\rho}{\mu} Vd$ (the Reynolds number); further, when RV was unity the unity ordinate supplied many constants which would not be found anywhere else. Upon that unity ordinate ($VR = 1$), $CR^{1.5} = \frac{1}{\sqrt{S}}$; $\rho \frac{P}{R} = \rho V^2 A$;

$C\sqrt{S} = V^{1.5}$ (the reciprocal of the Froude number); $P = Q$; and P gave the constant for a line drawn at 45 degrees to give readings of Q for any value of RV . The Author suggested that all such useful constants should be rejected in favour of the parameter $\frac{R_e}{7}$ which was no physical

* Correspondence on "The Reduction of Carrying Capacity of Pipes with Age." Journal Inst. C.E., vol. 9 (1937-38), p. 281 (October 1938).

Fig. 13.

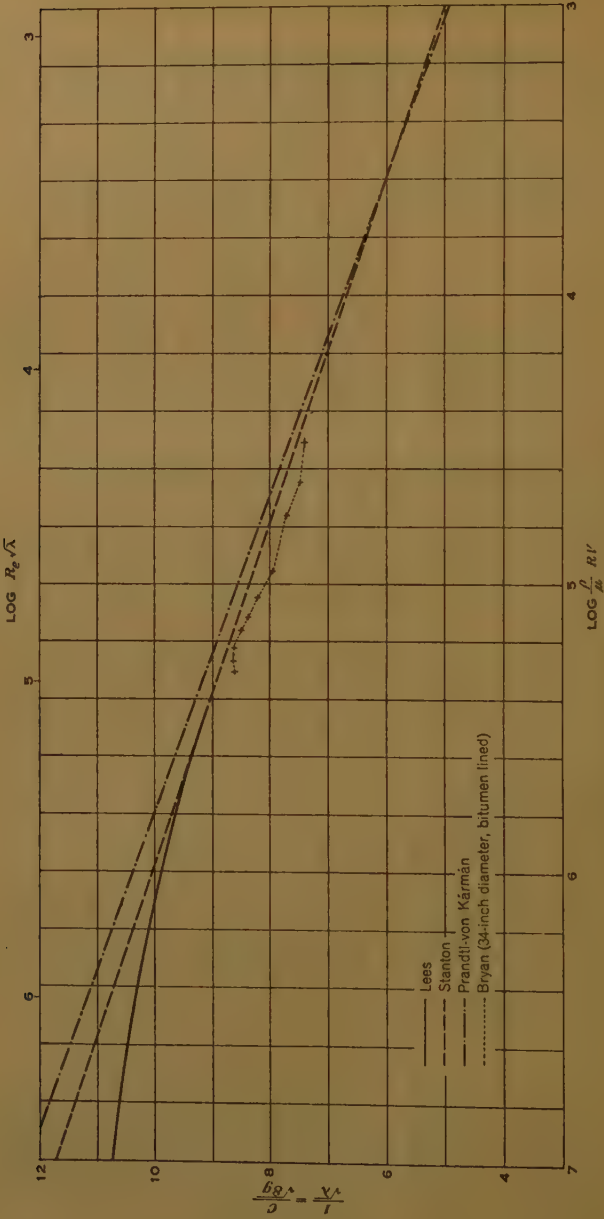


CHART ILLUSTRATING FIGURES IN TABLE IX.

function of fluid flow. To do so would be to reject all formulas which the engineer had become accustomed to using. One practical instance (shown on *Fig. 13*) was the 34-inch-diameter bitumen-lined main from Danbury to Herongate. That main was 69,639 feet long with thirteen valves and one hundred and ninety-two bends deviating 2,987 degrees. For a discharge of 24 cusecs, $\frac{Q}{P} = 2.71$; $\log \frac{R_e}{4} = 5.343$; and $\log \frac{R_e}{7} = 5.1$.

The Author gave $C = 148$, and loss of head $= 65.5 + 4.5$ (bends) $= 70$ feet; the recorded values were $C = 136.6$; $V = 3.81$; and total head lost $= 76$ feet. Analysing those figures, $A = \frac{136.6}{5.343} = 25.6$, which compared with

$A = 27$ for a straight pipe of the same length, whilst $76 \times \left(\frac{25.6}{27}\right)^2 = 68.5$ showing a loss of head of 7.5 feet to allow for bends, valves, or any other obstruction unwittingly overlooked by the Author.

The question would arise as to which was the most practical formula. Mr. Essex, after 30 years of study in the production of hydraulic formulas, was convinced that little reliance could be placed upon the results obtained from academic formulas, primarily on account of their failure to include, in some way, the constants covered by the Chezy function; that also constituted a weakness in the exponential type, which, if plotted to the von Kármán curve as suggested by the Author, would show still further disparity, but when plotted to the RV parameter would show some kind of comparison. That was demonstrated by Table X.

TABLE X.

Barnes reference No.	Materials.	Scale- factor: A	Value of Chezy C in Barnes formula to RV parameter.	Blench *.	
				C	A
XIV	Rock-faced masonry in cement.	19.4	$95R^{0.1775}.V^{-0.0375}$	$89 R_{\frac{1}{2}}$	17.3
XIII	Dressed masonry in cement.	26.4	$129R^{0.238}.V^{-0.035}$	$123 R_{\frac{1}{2}}$	25.6
XI	Hard brick conduit	26.0	$128R^{0.146}.V^{-0.073}$	$115 R_{\frac{1}{2}}$	21.4
X	Clean neat cement	32.5	$160R^{0.156}.V^{-0.033}$	$158 R_{\frac{1}{2}}$	33.2

It was well known that A. A. Barnes eliminated all trace of the Chezy number from his formula and records. When, however, the Barnes formula was replotted to the RV parameter, the Chezy value, so carefully

* "A New Theory of Turbulent Flow in Liquids of Small Viscosity" (Paper No. 5185, available in The Institution Library.—Sec. INST. C.E.). *Abstract published in Journal Inst. C.E., vol. 11 (1938-39), p. 611 (April 1939).*

excluded, reappeared and the limited scope of the formula was indicated. Its limitations for channels were clearly shown by the appearance of a negative index to the velocity values whenever the formula gave a scale-factor of 25 or greater. It was to be regretted that Mr. T. Blench * had, by using Barnes's Tables, deprived himself of the scale-factor by which he could have checked his formula. Had he used that check he could never have arrived at the erroneous figure of $A = 39.8$ for carefully-planed wooden boards. It was difficult to arrive at any comparison with the Author's scale-factor for smooth boards, but at $RV = 1$ it would certainly not exceed $\frac{135}{4.67} = 28.9$.

Mr. Essex referred to the many arguments arising from the academical variation of parameter (which, he thought, should clearly be $\frac{\rho}{\mu}RV$ if it were to combine the relationship of the Chezy function with the Reynolds function). He had attempted to show (Fig. 12, facing p. 404) the triviality of such arguments by marking the Author's line for $\frac{1}{\sqrt{\lambda}} = 1.8 \log \frac{R_e}{7}$ below his own line for $\frac{CR}{P} = 2 \log \frac{R_e}{4}$. The Author would observe that his line was non-dimensional whilst Mr. Essex's line was in foot-lb.-second units. To that, Mr. Essex would reply that the engineer required his drag values in British units; the ratio $\frac{CR}{P}$ was in such units and was more capable of extension in the investigation of useful constants than the Author's coefficient of resistance λ ; it was, indeed, as important as the ratio $\frac{V}{\sqrt{S}}$.

Comparison of the two lines, either of which was a function of the other, showed the close alignment of the result of 60 years' research work based on the theory of dimensions and Mr. Essex's extended researches into the fixed relationship between the Chezy number and the Reynolds number: in the one case, the wideness of the variety of academic formulas led only to doubt and confusion, whilst in the other case Mr. Essex had designed and published a calculator with adjustable scale for Chezy values which, for 30 years, had required no alteration.

Mr. J. R. Finniecome pointed out that, in recent years, the considerable advance in the knowledge of the friction coefficient in pipes was largely due to the Nikuradse tests at Göttingen. Those tests were specially considered in the Author's analysis. The friction loss was not only a function of the Reynolds number but was also principally dependent on the roughness factor. That had been particularly emphasized for the first time by Nikuradse, whose exhaustive tests were most valuable and

* Footnote (*), p. 409.

convincing. For a number of years Mr. Finnicome had been collecting interesting data from actual tests on the friction co-efficient λ for circular pipes for air, water, steam, and methane. Those were summarized in in Table XI (pp. 412-413) and *Fig. 14*. The experimental results pointed out, conclusively, that the roughness factor was of importance in determining the actual value of λ , particularly at high Reynolds numbers, such as were generally used for very large water-pipes. It was not possible to review in detail the formulas and the results given in Table XI and *Fig. 14* respectively; however, Mr. Finnicome felt that that summary would be useful and comprehensive.

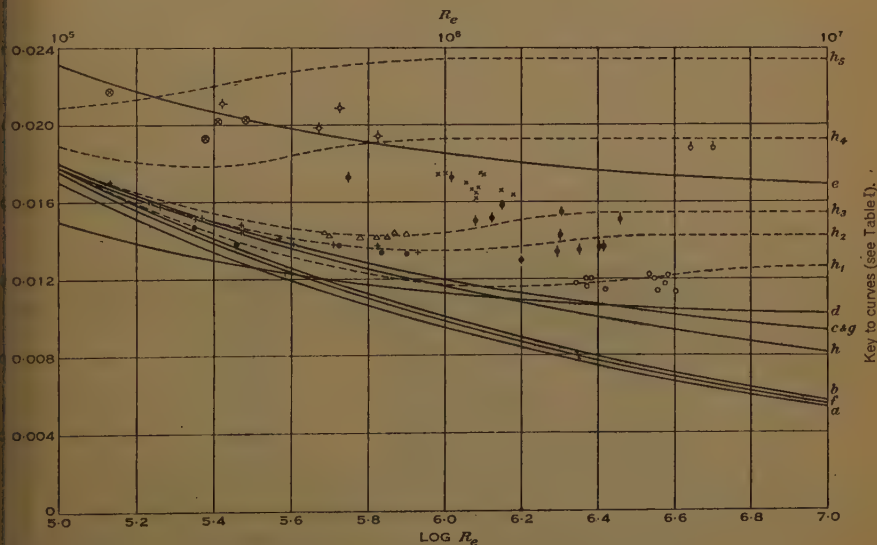
The friction coefficient was to be used in the following general formula:

$$\delta p = \lambda \cdot \left(\frac{V^2}{2g} \right) \cdot \frac{l}{d} \cdot \gamma \quad (1),$$

or

$$h = \lambda \left(\frac{V^2}{2g} \right) \frac{l}{d} \quad (2),$$

Fig. 14.



where V denoted the velocity in feet per second,

g was 32.17 feet per second per second,

l denoted the length of pipe-line in feet,





d „ bore of pipe in feet,

γ „ density in lb. per cubic foot,

δp „ pressure drop in lb. per square foot,

h „ loss of head in feet,

TABLE
COEFFICIENTS OF FRICTION FOR CIR-

Reference.	Curve or Symbol.	Author.	Year.	Medium.
1	a	Saph and Schoder	1903	Water
2	b	Blasius	1913	Water
3	c	Stanton and Pannell	1914	Air and water
4	d	von Mises	1914	
5	e	Lander	1915	Air, steam, and water
6	f	Jakob		
7	g	Jakob and Erk		
8		Prandtl-von Kármán-Nikuradse	1929 and 1933	Air, steam, and water
9	h	Nikuradse	1933	Air, steam, and water
10		Prandtl-von Kármán	1929	Air, steam, and water
11		Nikuradse	1933	Air, steam, and water
12	h_1	Nikuradse	1933 and 1935	Steam
13	h_2	Nikuradse	1933 and 1935	Steam
14	h_3	Nikuradse	1933 and 1935	Steam
15	h_4	Nikuradse	1933 and 1935	Steam
16	h_5	Nikuradse	1933 and 1935	Steam
17		Carnegie	1930	Steam
18		Carnegie	1930	Steam
19		Carnegie	1930	Steam
20		Dutch State Mines	1930	Steam
21	Δ	Bewag	1935	Steam
22	+	Bewag	1935	Steam
23	●	Bewag	1935	Steam
24	○	Bewag	1935	Steam
25	x	E. Guman	1930	Methane

REFERENCES TO BE READ IN

1. A. V. Saph and E. H. Schoder, "An Experimental Study of the Resistances to the Flow of Water in Pipes." Trans. Am. Soc. C.E., vol. li (1903), p. 253.
2. H. Blasius, "Das Aehnlichkeitsgesetz bei Reibungsvorgängen in Flüssigkeiten." Ver. dtsh. Ing. Forsch., 1913, Heft 131.
3. T. E. Stanton and J. R. Pannell, "Similarity of Motion in Relation to Surface Friction of Fluids." Phil. Trans. Roy. Soc., London, vol. 214 (1914), p. 199.
4. Von Mises, "Elemente der technischen Hydrodynamik." 1914.
5. C. H. Lander, "Surface Friction: Experiments with Steam and Water in Pipes." Proc. Roy. Soc. (Series A), vol. 92 (1916), p. 337.
6. M. Jakob, Zeitschrift Ver. dtsh. Ing., vol. 30 (1925).
7. M. Jakob and S. Erk, "Friction in Smooth Pipes and the Discharge of Standard Nozzles." Ver. dtsh. Ing. Forsch., 1912, Heft 267.
Also, Inst. C.E., Eng. Abs., Nos. 26-27 (January and April 1926), p. 21.

XI.

ULAR PIPES, BASED ON TESTS.

Formula for λ .	Remarks.
$0.3164 R_e^{-0.254}$	1.255-12.62-centimetre bore solid-drawn smooth tubes. Brass pipes. For smooth pipes. Small commercial wrought-iron pipes.
$0.3164 R_e^{-0.25}$	
$0.0072 + 0.612 R_e^{-0.35}$	
$0.0096 + 1.7 R_e^{-0.5}$	
$0.016 + 1.128 R_e^{-0.44}$	
$0.327 R_e^{-0.254}$	For smooth pipes.
$0.00714 + 0.6104 R_e^{-0.35}$	
$\frac{1}{\sqrt{\lambda}} = 2 \log_{10} \left(\frac{R_e \sqrt{\lambda}}{2.51} \right)$	For smooth pipes.
$\frac{1}{\left[1.8 \log_{10} \left(\frac{R_e}{7} \right) \right]^2}$	For rough pipes.
$\frac{1}{\left[2 \log \left(3.7 \frac{d}{k} \right) \right]^2}$	For rough pipes.
$\frac{1}{\left[1.74 + 2 \log \left(\frac{r}{k} \right) \right]^2}$	$\frac{r}{k} = 15 \text{ to } 4,000$
—	14-inch bore, $\frac{r}{k} = 3,400$.
—	10-inch bore, $\frac{r}{k} = 2,000$.
—	7-inch bore, $\frac{r}{k} = 1,400$.
—	$\frac{r}{k} = 507$.
—	$\frac{r}{k} = 252$.
—	8-inch diameter, solid drawn.
—	6-inch diameter, hot rolled.
—	1.98-inch diameter, lap welded.
—	7-inch bore.
—	7-inch bore.
—	10-inch bore.
—	10-inch bore.
—	14-inch bore.
—	10-inch-bore, 48.6-kilometre pipe-line for Sarmas-Turda.

CONJUNCTION WITH TABLE XI.

- 8, 9, 10, and 11. J. Nikuradse, "Strömungsgesetze in rauhen Röhren." Ver. dtsh. Ing. Forsch., 1933, Heft 361.
- 12, 13, 14, 15, 16, 21, 22, 23, and 24. W. E. Wellman, "Städteheizung." Zeitschrift Ver. dtsh. Ing., vol. 79 (1935), p. 767.
- 17, 18, and 19. F. Carnegie, "The Economical Production and Distribution of Steam in Large Factories." Proc. I. Mech. E., 1930, vol. 1 (January-May), p. 473.
20. Dutch State Mines. Die Wärme. 29 March 1930.
25. E. Guman, "Zur Bestimmung der Reibungszahl in Ferngasleitungen." Zeitschrift Ver. dtsh. Ing., vol. 74 (1930), p. 107.

In *Fig. 15* the friction coefficient was plotted as a function of the Reynolds number,

$$R_e = \frac{Vd}{\nu}, \quad (3)$$

where ν denoted the kinematic viscosity in feet per squared second.

Fig. 15.

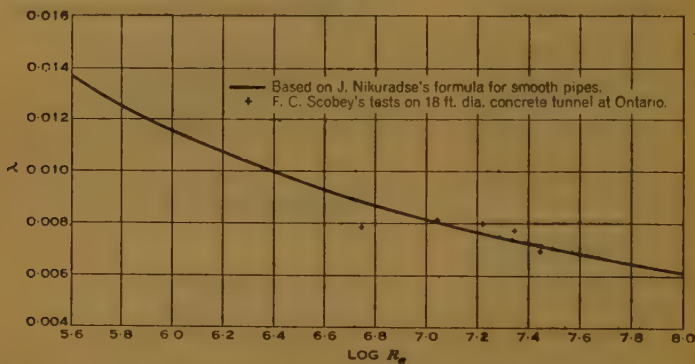
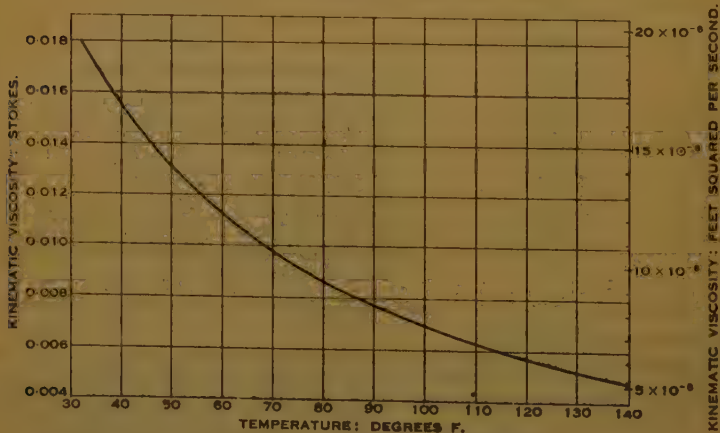


Fig. 16 represented graphically the kinematic viscosity at various water temperatures expressed in stokes when using centimetre-gram-second units, and in feet squared per second for foot-pound-second units,

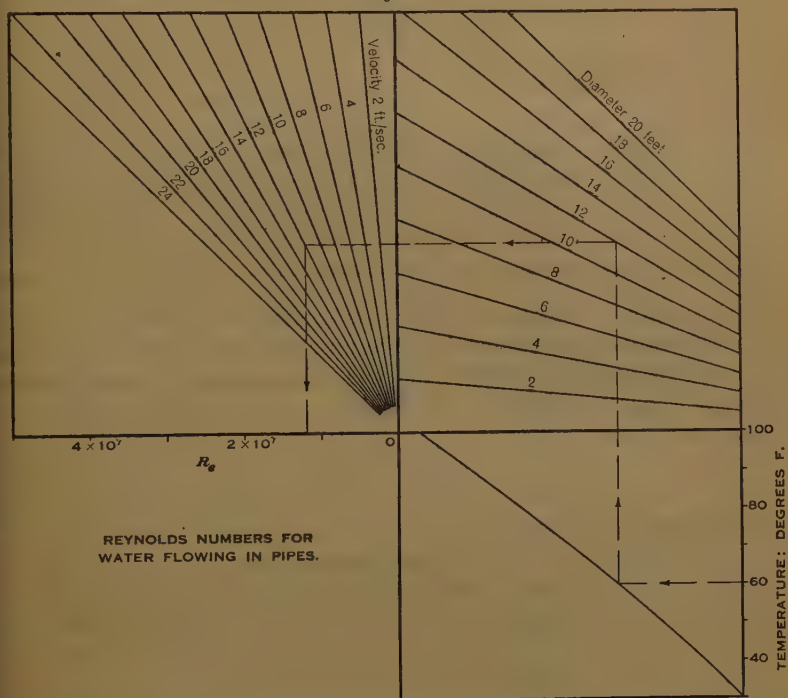
Fig. 16.



when applying the formula (3). Further, the chart (*Fig. 17*) had been specially prepared to assist engineers in determining, quickly, the Reynolds number at various diameters, velocities, and temperatures. Mr. Finnicome was particularly interested in the test carried out by F. C. Scobey on the

18-foot-diameter concrete tunnel at Ontario and in the values for λ recalculated and corrected by the Author. Those results agreed very closely with the Nikuradse formula for smooth pipes, as shown by the Author in *Fig. 2* (p. 144 §), and by Mr. Finnicome in *Fig. 15*. *Fig. 15* enabled the value for λ to be read directly and indicated, also, that the value of λ for test No. 1 was slightly lower than the curve for smooth pipes. Mr.

Fig. 17.



REYNOLDS NUMBERS FOR
WATER FLOWING IN PIPES.

Finnicome had compared Scobey's test values with the empirical formula based on the extensive tests on concrete pipe-lines carried out by A. A. Barnes*. Those values were given in Table XII (p. 416), column 6, and the value for λ was derived from A. A. Barnes's basic formula and corresponded to

$$\lambda = \frac{0.02329}{d^{0.309} V^{0.182}},$$

and was based on

$$h = 0.0003617 \frac{V^{1.818}}{d^{1.309}} \cdot l,$$

§ *Ibid.*

* "The Discharge of Pipes Lined with Concrete or Bitumen." Trans. Inst. W.E., vol. xxxviii (1933), p. 158.

where V , d , l and h were in foot-second units. The ratio of the test values to those obtained by A. A. Barnes's formula were shown in Table XII, column 7. It would be found that the test friction coefficient was, on an average for the last four tests, about 28 per cent. higher than Barnes's results.

TABLE XII.—ANALYSIS OF F. C. SCOBEE'S TEST ON 18-FOOT-DIAMETER CONCRETE TUNNEL AT ONTARIO.

1	2	3	4	5 6		7	8
Test No.	Velocity: feet per second.	Reynolds number: R_e .	Log R_e .	λ		(5) (6)	n
				Tests.	Barnes.		
1	4	5,550,000	6.7443	0.00782	0.00741	1.0555	0.01055
2	8	11,100,000	7.0453	0.00812	0.00654	1.242	0.01070
3	12	16,665,000	7.2217	0.00798	0.00607	1.316	0.01060
4	16	22,200,000	7.3464	0.00773	0.00576	1.342	0.01045
5	20	27,700,000	7.4425	0.00697	0.00554	1.257	0.00900

Furthermore, it was interesting to determine, from Scobey's results, the constant n used in the Gauckler-Manning-Strickler formula which was also in general contemporary use and was expressed by the basic formula published originally in 1867:

$$V = \frac{1}{n} R^{\frac{2}{3}} \left(\frac{h}{l} \right)^{\frac{1}{4}},$$

where V denoted the velocity in feet per second,

R ,, hydraulic mean radius in feet,

h ,, loss in head in feet,

l ,, length in feet,

$\frac{h}{l}$,, loss in head per unit length.

From that formula the friction coefficient λ became

$$\lambda = 185.8 \frac{n^2}{\sqrt[3]{d}},$$

where d denoted the inside diameter of the pipe in feet. That value for n , based on the results of λ for the 18-foot-diameter concrete tunnel, was given in Table XII, column 8. It would be found that n varied from 0.009 to 0.0107 and that for the tests Nos. 1, 2, 3, and 4 the average value for n was 0.0106.

Mr. Gerald Lacey urged a simplification of the notation used in the Paper. Since the hydraulic mean depth was fundamental to all flow problems it was desirable that it should be substituted for the diameter of the pipe. Further, since the density and the viscosity were always asso-

ciated together in the Reynolds number it was preferable to employ the kinematic viscosity.

The Reynolds number quoted by the Author,

$$\frac{\rho U d}{\mu},$$

could then be re-written as

$$\frac{(Um)}{\nu}.$$

In that form the Reynolds number was more descriptive. The product in the brackets was equal to the discharge per unit length of the wetted perimeter, a very important variable in open flow in alluvial channels. Mr. Lacey denoted that product by q , the "discharge intensity." Thus :

$$\begin{aligned} q &= \frac{Q}{P}, \\ &= \frac{PUm}{P} = (Um). \end{aligned}$$

The Reynolds number thus appeared to be the "discharge intensity" divided by the kinematic viscosity, or the "mass discharge intensity" divided by the viscosity. In either case the simplest algebraical form of the Reynolds number was q/ν . That number was of universal application and appeared preferable, since the simplified number was more readily endowed with a physical significance. It was necessary to think only in terms of the product Um and not of the factors. Discharge was the basis of scale effect in alluvial channels and all model-work, and it was desirable to deal in terms of discharge, and discharge intensity, whenever possible.

The use of the coefficient λ by the Author appeared to arise from the traditional empirical analysis of pipe-flow data rather than from fundamental considerations. The "shear-force velocity" could be substituted and λ eliminated. Thus :

$$\begin{aligned} \lambda^{-\frac{1}{2}} &= \left(\frac{U}{V_*} \right) \frac{1}{8^{\frac{1}{2}}}, \\ &= 0.353 \left(\frac{U}{V_*} \right). \end{aligned}$$

Mr. Lacey thought that the ratio U/V_* had much to commend it. Had that number been used in plotting *Fig. 4* (p. 147 §), it would have been evident, immediately, that V_* occurred on both sides of the relation.

Equation (3) (p. 135 §) was numerically very simple, but not quite so

simple on analysis. Would the Author re-write it in terms of U/V_* , natural logarithms, and the ratio m/k ? Since all the dimensions had been taken into account, any residual numerical constant could be associated only with the geometrical properties of a spherical or hemispherical obstruction.

The Reynolds number, written as Mr. Lacey suggested, and the Reynolds roughness number, presented an interesting analogy. Thus:

$$\frac{Um}{\nu} = q/\nu,$$

and

$$\frac{V_*k}{\nu} = q_*/\nu.$$

The mean velocity multiplied by the hydraulic mean depth resulted in a "discharge intensity" common to the entire cross-sectional area. Similarly the "shear-force velocity" multiplied by the "roughness mean depth" resulted in a second discharge intensity denoted by q_* . That discharge intensity, which, to be consistent should be termed the "shear-force discharge intensity", was common to the boundary area which was a function of P and k .

If the relative roughness k/m were substituted for the relative roughness k/d , it would be clear that k/m was the ratio of the cross-sectional areas associated with the "shear discharge" and the "discharge" respectively.

The Author had pointed out that the roughness Reynolds number was a product of three non-dimensional numbers: the resistance coefficient, the relative roughness, and the Reynolds number. Was it possible to assign a less complicated significance to the roughness Reynolds number?

It would appear that the entire problem could be re-stated in more general terms by using, as "arguments", the ratios

$$V_*/U, k/m, q/\nu, \text{ and } q_*/\nu.$$

It would then be possible to approach the problem of open flow without being hampered by coefficients associated with a specialized form of flow.

Mr. Lacey observed that it would be unwise to conclude that the Author's equations could be applied, without modification, to the problem of flow in open alluvial channels generating their own cross sections in conformity with their discharges. The Paper was essentially a self-contained contribution to the problem of flow in pipes.

Mr. G. G. McDonald drew attention to an approximate transition formula which he had suggested*. That formula was:

$$\lambda = c_1(c_2/R)^\beta, \quad \dots \dots \dots (a)$$

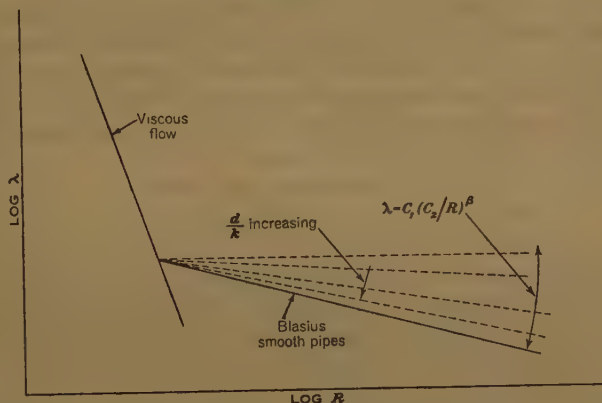
where

$$\beta = \frac{[1 - \{c_3 \log (c_4 d/k)\}^{-1}]}{[4 - \{c_3 \log (c_4 d/k)\}^{-1}]} \quad \dots \dots \dots (b)$$

* "Efficiency Formulae for Model and Full-Scale Centrifugal Pumps, &c." *The Engineer*, vol. clxvii (1939), p. 285 (March 1939).

For pipe flow, suitable values of the coefficients were $c_1 = 0.055$, $c_2 = 1,122$, $c_3 = 0.77$, and $c_4 = 0.5$ nearly, and the equation when plotted to logarithmic co-ordinates gave straight lines radiating from the point of intersection of the viscous and Blasius's lines, as shown in *Fig. 18*. In the

Fig. 18.



case of very smooth surfaces, that was to say, when $d/k = \infty$, equation (a) reduced to the Blasius's form for smooth pipes.

It might be pointed out that the equation (a) was a development of the equation

$$\lambda = c_5(R)^\alpha, \quad \dots \dots \dots (c)$$

which was used by Mr. R. J. S. Pigott † in his analysis of many cases of flow in rough pipes. Both c_5 and α varied with roughness.

Mr. James Williamson observed that the Paper was an industrious attempt to solve a problem involving a complex combination of variables. Roughness-pattern appeared to be a material factor as well as the absolute roughness. That further complicated the issue.

The term "hydraulically smooth" had not been defined, but might be taken as the smoothness of a surface similar to Nikuradse's "glatt" surfaces, that was to say, the smoothness of small drawn tubes without joints. The roughness pattern in such tubes might be directed in longitudinal lines by the drawing.

Inspection of *Figs. 3, 4, 5, 6, 7 and 8* (pp. 146 §, *et seq.*) disclosed no indication of any appreciable extent of smooth-pipe law. It was probable that the joints in any commercial pipe-line produced sufficient gross turbulences to affect the flow past the smooth surfaces. The d/k values given on some of the diagrams would indicate extremely smooth conditions

† "The Flow of Fluids in Closed Conduits." *Mechanical Engineering*, New York, vol. 55 (1933), p. 497 (August 1933).

§ *Ibid.*

such as should produce the smooth-pipe law at the lowest velocities. In some of the diagrams the line designated "smooth law" had been extended to the left beyond the limit of the range of that law (about $R_e = 5,000$) or $R_e\sqrt{\lambda} = 1,000$).

Examination of Tables II, III, IV and V (facing p. 156 §) indicated that the rough-law (V^2 -law) applied to none of those pipes except one or two of the small ones at velocities ranging from 20 to 30 feet per second. That was indicated by the coefficient C increasing continuously with increase of velocity. That was not in agreement with a large body of experience with many practical pipes, where the V^2 law giving constant C was found to apply beyond moderately low velocities.

In a recent Paper*, Mr. Williamson had attempted to show that the formula $S = \frac{V^2 n^2}{2 \cdot 2 R^{1.33}}$ had been proved reliable for a wide range of large-pipe conditions and that it also applied very closely to the limited range of Nikuradse's small-pipe experiments in the rough-law region. By that formula, k was found to vary as n^6 and checks were found which established reasonable consistency over a range of more than a thousand degrees of roughness. The range of degrees of roughness in Nikuradse's experiments was only sixteen (from 0.01 centimetre to 0.16 centimetre).

The above formula might be put in the non-dimensional form,

$$S = 0.18 \left(\frac{k}{d} \right)^{\frac{1}{3}} \cdot \frac{V^2}{2gd^5} \quad \dots \quad (1)$$

from which

$$\lambda = 0.18 \left(\frac{k}{d} \right)^{\frac{1}{3}},$$

whereas the rough-law adopted by the Author gave

$$\frac{1}{\sqrt{\lambda}} = 2 \log R\sqrt{\lambda} \quad \dots \quad (2).$$

Mr. Williamson concluded, from careful analysis of Nikuradse's data, that the trend of formula (2) at the larger values of $\frac{d}{k}$ was likely to be influenced by the enormous difficulty of determining, even to an approximation of 25 per cent., the true value of k in the inaccessible middle section of a small pipe, and by an error in calculation which pervaded one section of Nikuradse's investigation.

The wide difference in trend of the formulas (1) and (2), at values of $\frac{d}{k}$ beyond Nikuradse's range, was shown in Table XIII, where values of $\frac{1}{\sqrt{\lambda}}$ greater than 7 were beyond that range.

§ *Ibid.*

* "Considerations on Flow in Large Pipes, Conduits, Tunnels, Bends, and Siphons." Journal Inst. C.E., vol. 11 (1938-39), p. 451 (April 1939).

TABLE XIII.

λ	$\frac{1}{\sqrt{\lambda}}$	Values of $\frac{k}{d}$ calculated from:	
		Formula (1) (derived from Manning).	Formula (2) (Nikuradse).
0.0625	4	24	27
0.0400	5	90	85
0.0278	6	270	270
0.0204	7	682	850
0.0156	8	1,540	2,700
0.0123	9	3,100	8,500
0.0100	10	5,800	27,000
0.0079	11.2	11,700	107,000

The Author had made considerable use, for the purpose of his analysis, of values of k calculated from formula (2), and a pertinent criticism of a diagram such as *Fig. 4* (p. 147 §) was that the k values used were calculated values, not experimental values. By that procedure the plotting would show a measure of consistency with the formula. That formula (2) could produce extremely small roughness values might be illustrated by the figures corresponding to $\lambda = 0.0079$, in Table XIII, which was the mean value for the greater part of the velocity range in the 18-foot-diameter concrete-lined Ontario tunnel, and corresponded closely to the V^2 law. By formula

(1) the value of k was $\frac{216}{11,700} = 0.019$ inch, which was reasonable for a smooth concrete having a value of n of about 0.0105. By formula (2)

(Nikuradse) the value of k arrived at would be $\frac{216}{107,000} = 0.002$ inch, which was obviously too small for any concrete surface. Presumably there was a slip in the scale of sizes at the left of the lower diagram of *Figs. 9*, p. 152 §, and that to maintain progression the two lower figures should have been 0.0004 and 0.0006, instead of 0.004 and 0.006. If that were correct, then the Author had obtained calculated roughness values for certain wrought-iron pipe surfaces amounting to less than 0.001 inch, which was a scarcely credible degree of smoothness for a rolled-strip surface.

Mr. Williamson concluded that: (a) Results derived from Nikuradse's smooth pipe and rough pipe investigations should not be applied to conditions outside the character and range of the laboratory experiments.

(b) There was good evidence that the Nikuradse rough-pipe law would not apply for $\frac{d}{k}$ values above 500.

(c) There was no theoretical basis for the Author's transition law, although it provided empirical indication of a general trend.

(d) The Author was wise to discard the formula as being of limited service and awkward to use.

(e) In Tables III, IV and V (facing p. 156 §) the coefficient C was generally too high for safe practical use, and over large sections of the Tables the progression of C from small to large pipes at a constant velocity, or from moderate to large velocities in a pipe of constant size, did not agree with much of the experience accumulated for pipes outside the laboratory scale.

* * * Owing to the outbreak of hostilities, the Author's reply has not been received in time for insertion here. It is hoped to publish it later.—SEC. INST. C.E.

§ *Ibid.*

Paper No. 5190.

“Anti-Malarial Operations in the Delhi Urban Area.”†

By ARTHUR WILLIAM HENRY DEAN, M.C., B.Sc., M. INST. C.E.

Correspondence.

Lieutenant-Colonel Gordon Covell (Director, Malaria Institute of India) observed that, in the planning of anti-malarial measures in Delhi, it was essential that the liability of that part of India to periodical visitations of the disease in what was known as its regional, or fulminant, epidemic form be constantly kept in mind. Failure to do so was to court disaster.

In certain years, and in certain areas in particular years, the usual increase in malaria prevalence which occurred every autumn was enormously exaggerated. The total number of deaths occurring in areas coming within the epidemic zone might then be from 10 to 20, or even 30, times the normal. Fortunately, epidemics of such terrible intensity were rare, and no major epidemic had visited Delhi since 1908. There were, however, minor epidemics, which occurred at intervals of about from 5 to 7 years, in which the prevalence of malaria was very markedly increased and became a public-health problem of considerable gravity. In an epidemic year, not only was there a great increase in the number of malaria cases, but the actual virulence of the infection was much enhanced, so that attacks of fever were much more severe than in other years. Furthermore, there was a great increase, not only in the number of mosquitoes, but also in the percentage of mosquitoes which were infected with malaria parasites.

† Journal Inst. C.E., vol. 11 (1938-39), p. 157 (February 1939).

Epidemics tended to occur especially in years of excessive monsoon rainfall following a series of years in which the rainfall had been in defect, provided also that the level of the general immunity of the population against malaria was low. For that last reason, they did not occur in the same locality at intervals of less than 5 years, even in years of high rainfall. After the lapse of 5 or more years since the last epidemic a large number of children were present in the community who had never been exposed to malarial infection, whilst those who had been infected during the last epidemic had lost practically all their immunity. Conditions were therefore suitable for the advent of another fulminant outbreak. In Delhi the last two epidemics occurred in 1926 and 1933. The monsoon rainfall in 1937 and 1938 was abnormally low, so that an epidemic of malaria was very likely to occur in the next year in which there was a copious monsoon rainfall.

Malaria-control measures could be divided into two classes: namely, permanent works designed to eliminate breeding places; and recurring measures, such as the application of larvicides and minor levelling and draining operations, which had to be continued year after year, just as streets had to be scavenged, or fruit-trees sprayed to kill insect pests. It was primarily for the mitigation of epidemic malaria that the programme of permanent anti-malaria works in Delhi was planned in 1936. It was hoped that when they were completed the incidence of the disease would be kept in check by recurring measures, even in an epidemic year. About half of the programme had been executed, but until it had been completed it could not be said that the problem of malaria-control in Delhi had been adequately tackled. It had been hoped that the entire programme would have been completed before the advent of the next epidemic year, but unfortunately financial stringency had held it up, and it was practically certain that Delhi would experience another epidemic before the remaining works were taken in hand. The completion of the programme was of more than local interest, since it would provide an indication of the extent to which permanent engineering works could influence epidemic malaria.

Mr. H. E. FitzGibbon had previously shown* that there was an infallible correspondence between the rainfall- and malaria-curves for every malarial area, and had suggested that a rising subsoil-water level, following the seasonal rains or melting snows on adjacent mountains, highlands, or plateaux, might supply the hitherto missing explanation for the seasonal vagaries of the miasma. That malaria "loved the ground", and was intimately related to fluctuating subsoil-water levels, were facts that had been observed and commented upon by the very highest authorities in the orthodox medical profession; whilst the pumping, or more accurately the breathing, action of the earth, in drawing down air

* "Malaria. The Governing Factor." The C. W. Daniel Company, London, 1932.

into the soil, and, after a lapse of time and under the influence of rising subsoil water, in exhaling poisonous heavy gases which were present in the soil, was a commonplace feature of sanitary engineering. To that simple explanation should be added the observations of experts and commissions, testifying to the utter disregard of malaria for anopheline mosquitoes in many places, and of anopheline mosquitoes for malaria in as many others; to the presence of the parasite without the disease and of the disease without the parasite in numerous instances; and to the total eradication of the disease by engineering works comprising flooding in some places and deep drainage in others, without any assistance from the medical authorities. In view of that, it was hardly too much to expect that the *prima facie* relationship of malaria to subsoil-water fluctuations should receive serious consideration.

In a country such as India, where wells were numerous, it would be a very simple matter to procure exact records of subsoil-water levels over a prolonged period; and by plotting those together with the figures for malaria incidence in each district in which the well water-levels were recorded, material for arriving at last at a sound conclusion would be available. That, in turn, would lead to the adoption of efficacious engineering measures and to the elimination of wasteful expenditure in a useless war against swarms of insects and micro-organisms. It would be necessary to eliminate errors due to artificial interference with well water-levels, and to the possible inclusion of old or prophylactic-treated malaria cases in the incidence Tables; but with a little care the required accuracy could be ensured to a degree (not necessarily absolute) sufficient for all reasonable purposes.

**** Professor Raja Ram**, of Roorkee, United Provinces, observed that one of the earliest steps necessary in the anti-malarial campaign was to find out the location of the breeding places of the mosquitoes and to fix catching stations for the insects. There were six species of anophelines found in Delhi, of which only two were malaria-carriers in that area. "Quimby" plates were then erected to indicate the direction of flight of the mosquitoes. The spleens of thousands of children were examined both before and after the campaign, and gave the following results:—controlled areas: before the campaign, 29–56 per cent., after the campaign, 15–26 per cent.; uncontrolled areas: before the campaign, 34–51 per cent., after the campaign, 53–89 per cent.

The principal factors affecting malaria in Delhi at that time might be classified as followed:—

(1) Excessive canal irrigation, coupled with interference with natural drainage by railway, road, and canal embankments in the North Western section.

**** Professor Ram's** contribution has been abstracted from a lengthy communication describing the anti-malarial operations; the MS. and illustrations may be seen in the Institution Library.—SEC. INST. C.E.

(2) The annual flooding of the Bela (the foreshore of the Jumna) during the rainy season, leading to the heading up of water in the various storm-water drainage channels that discharged into the Jumna, and to the formation of prolific breeding places as the flood receded.

(3) Interference with natural drainage in New Delhi by the presence of the Kilokri sewage-farm, which was completely waterlogged and had now been closed down and replaced by a new sewage-disposal works, of the Agra-Delhi Chord Railway embankment, and of brickfields.

(4) The presence of a vast number of excavations throughout the area, in the form of borrow-pits alongside railways and roads, and of pits in brickfields and quarries, in which water collected and afforded dangerous mosquito-breeding places.

(5) Numerous defects in the underground drainage system, more especially in New Delhi.

(6) The presence of numerous uncovered wells and non-mosquito-proof or open cisterns.

(7) Mosquito-breeding in water-collections fed from hydrants, and in ornamental fountains.

In general, those factors in the control of malaria in Delhi could be reduced to only three major heads.

(I) The breeding of mosquitoes in the Bela or foreshore of the Jumna, which was very difficult to control. At one time, before operations were commenced, the only solution suggested, and accepted, was treatment with "Paris Green" from an aeroplane.

(II) The canal-irrigated area round about Sabzimandi, which was an important breeding place for mosquitoes of all kinds.

(III) Miscellaneous sources, such as breeding in open areas, partially open wells, cisterns, tanks, closets, flushing tanks, storm-water drains in New Delhi, anti-formicas in the houses, water collecting round hydrants for which soakage pits had been recommended, pits, depressions, etc.

Both permanent and temporary control measures had been started in the areas. A cheap and efficient anti-mosquito spray and anti-mosquito pomade were also made available at the Municipal Health Offices. An endeavour was made to induce the inhabitants of Delhi Municipality to have their wells covered and fitted with hand pumps, not only as an anti-mosquito measure but also as a measure of general sanitation, by offering to meet half of the cost of doing so for private houses in Kashmere Gate. That offer remained open for several months, but it met with a very poor response.

In the New Delhi area the siphons in storm-water drains were to be replaced by straight channels; cisterns and wells were to be made mosquito-proof; and all surface water was to be carried to the underground storm-water drains by means of offsets 12 feet or more long. It had been proposed to seal the storm-water drainage system by the provision of flaps at all manhole connexions. Professor Ram, however, was of the opinion that

flaps on storm-water drains required considerable attention, which it was extremely difficult to give, and he was therefore opposed to the provision of flaps. Each division of the area was put in charge of a malaria inspector, assisted by a mate and the requisite number of coolies. Under the Malaria Officer, New Delhi, a system of weekly house inspection was brought into force. It was intended to control effectively the breeding of mosquitoes in houses.

The limit of control on the Bela was fixed at $\frac{1}{2}$ mile from habitation, and having demarcated the boundary line for control measures on the riverside a station had to be fixed for the recording of flood-levels in the Jumna and the resultant flooding of the Bela. In attempting to deal with the problem of breeding in the Bela, and consequently of some kind of control of the Jumna river, it was found that many Government Departments were concerned owing to the local conditions. At Professor Ram's suggestion the Government of India was asked to appoint a committee of experts, composed of both engineers and health officers, representative of the interests of the administrations and provinces likely to be affected in any way by the anti-malarial operations. The proceedings of that committee had been printed¹, and would be of interest to engineers, more especially as all malaria in Delhi was man-made.

As the result of the deliberations of that committee a programme of projects for combating man-made malaria in Delhi was approved.

Returning to the problem of the Bela, it was later discovered that the Jumna had originally run by the fort walls, so that there was formerly no Bela or foreshore; if there were then breeding places of mosquitoes in the bed of the Jumna, they were on the other side of the river, probably at a distance of at least 1,500 feet. By tampering with the flow of the Jumna by the construction of a bridge, the Okhla dam, and the Hindun cut, the Bela had been unwittingly created, and with it the breeding of mosquitoes.

The Author, in reply, observed that records of the rise and fall of subsoil water as observed from wells in the Delhi Urban area were maintained. As a result of the very extensive irrigation of the area that had followed the creation of a garden city, the subsoil-water level had risen considerably; that had not, however, been accompanied by an increased incidence of malaria, which had, since the remedial measures described in the Paper, fallen to a marked degree.

The reason why the presence of a relatively high subsoil-water level was frequently associated with malaria was that that high subsoil-water level was always associated with high humidity. Under those conditions mosquitoes lived longer, and hence they were able to spread malaria to a greater extent than under drier conditions when their life was shorter.

The question of the prevalence of malaria when there were very few

¹ "Malaria Control in Delhi." Report of Committee of Experts. Government of India: Department of Education, Lands, and Health, 1936.

anopheline mosquitoes in any neighbourhood, and alternatively the relative absence of malaria when there were a large number of anopheles, was a question which was still under investigation by the malariologists. Investigations had shown that in some of the former cases—that was to say, where the number of anopheles was small, but nevertheless a high degree of malaria was prevalent—as many as 20 per cent. of the anopheles present might be infected with the malaria parasite; that was to say, the infectivity-rate was high. In the other set of circumstances the percentage of infected anopheline mosquitoes might be as low as 0.04.

Another explanation of that sort had been found, particularly in Europe, where mosquitoes of the same species had been found to belong to many separate races, some of which were anthropophilic, feeding exclusively on man, and others zoophilic, feeding exclusively on animals. It was obvious that the prevalence of the latter variety in a locality would not lead to any human malaria.

Again, it had been found that a certain species might in some parts of the world act as a carrier, whilst at some comparatively short distance away exactly the same species was found never to carry the malaria parasite. Examples of that were found in India, Java, and Malaya. The problem was thus both fascinating and complex, and it was not easy in the existing state of knowledge to present a complete story of malaria transmission. Nevertheless, to reject the mosquito-borne theory on that account would be a retrograde step, as it would mean ignoring an overwhelming mass of positive evidence that had accumulated all over the world.

The ingress of mosquitoes to the underground storm-water drainage system was best prevented by having long offsets from the grating to the main drains. By experiment it had been found that mosquitoes would not pass through a relatively long pipe; the length that stopped them varied with the diameter, and offsets of the requisite length were now given to all surface connexions. That did away with the necessity for flap valves.

It was true that the incidence of malaria had in many places been increased by engineering works carried out before the etiology of the disease was understood, but now that it was more widely known, engineers were able to design their works so as not merely to avoid increasing malaria, but to reduce it to readily controllable proportions.

The interest that the question of the relation between engineering works and the incidence of malaria had recently aroused was shown by two recent Reports*.

* "Malaria Control for Engineers." Report of the Joint Committee of the National Malaria Committee through its Sub-Committee on Engineering, and the Sanitary Engineering Division, American Society of Civil Engineers. Proc. Am. Soc. C.E., vol. 65 (1939), p. 229. (February 1939.)
Majors H. W. Mulligan and M. K. Afridi, "The Prevention of Malaria incidental to Engineering Construction." Malaria Institute of India Health Bulletin No. 25, Malaria Bureau No. 12. New Delhi, 1938.

Professor Raja Ram was associated with the proposals for permanent engineering works to control the incidence of malaria in the Delhi Urban area, as the Engineer of what was then called the Malaria Survey of India. Some of the schemes that he mentioned had been adopted and others had not been found necessary.

A still more extensive programme of works, estimated to cost Rs.704,650 in the year 1940-41, and Rs.1,051,520 in the year 1941-42, had recently been submitted to Government by Lieutenant-Colonel Gordon Covell, C.I.E., I.M.S., Director of the Malaria Institute of India, and the Author, and it was expected that funds would be found for those measures, which would, it was hoped, finally eradicate malaria from the Delhi Urban area, subject, of course, to regular recurring maintenance.

CORRESPONDENCE
ON PAPERS PUBLISHED IN
MARCH 1939 JOURNAL.

Paper No. 5188.

“The Gorge Dam.”†

By WILLIAM JAMES EAMES BINNIE, M.A., and
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Correspondence.

Mr. I. E. Houk, of Denver, believed that the upstream deflexions (p. 195 §) of the lower part of the thrust block during the filling of the reservoir were due, principally, to downward elastic movements of the rock formations in the bottom of the canyon above the dam, those downward elastic movements being caused by the accumulating reservoir load. Similar upstream deflexions during increasing reservoir stages had been observed at dams in the United States, particularly at Norris dam. Norris dam was a concrete gravity dam, 265 feet high at the maximum section, located on Clinch river near Knoxville, Tennessee. It had been designed by the Bureau of Reclamation and built by the Tennessee Valley Authority. Construction operations had been completed in 1936.

At Norris dam, an increase in reservoir stage of approximately 125 feet, during the months of March and April, 1936, had caused an upstream deflexion of about 0.15 inch at the top of the dam. It seemed probable that all of that deflexion was due to elastic deformation of the dolomitic rock formations in the lake bed upstream from the dam, caused by the increasing weight of the stored water. During the succeeding months of May to August, the upstream deflexion had increased gradually to a maximum of about 0.30 inch, the reservoir surface remaining at approximately the same level during that period. Although some lag between load application and maximum deflexion was not unusual, it was believed that the major portion of the increased upstream movement during May to August had been caused by temperature conditions. Norris dam was

† Journal Inst. C.E., vol. 11 (1938-39), p. 179 (March 1939).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

located in an east-to-west direction, with its downstream slope facing the south. Relatively cold water, accumulating at the upstream or north face of the dam, had cooled the concrete near the water face and had caused it to contract, while warm rays of the sun, shining on the downstream or south slope of the dam, had heated the concrete near the exposed face and had caused it to expand.

Considering the lag between maximum reservoir stage and maximum upstream deflexion of the lower part of the thrust block at the Gorge dam, as shown in *Fig. 10* (p. 196 §), it was possible that temperature changes might have been partly responsible for the upstream deflexions. However, from Mr. Houk's limited information regarding weather conditions at Hong Kong, he was inclined to believe that foundation deformations had been primarily responsible for the upstream movements.

Mr. G. B. Gifford Hull observed that the Paper dealt chiefly with matters of design, and that it might be of interest to supplement it by reference to some of the construction features.

The quantity of stone placed in the rock-fill was given in the Paper as 480,000 cubic yards or, with the 30 per cent. of voids which it was hoped to obtain by the method of hand-packing, and allowing for the settlement which would take place, about 700,000 tons. The progress of the construction was planned with a view to giving water to Hong Kong in the shortest possible time, in fact, 1 year earlier than the date by which it had been promised. To that end, methods of quarrying for rock-fill were designed to give a minimum figure of 1,000 tons per day. The usual method of drilling holes about 20 feet deep, and firing thirty or forty simultaneously, was adopted, and was continued for some months, but, having regard to the fact that in the rainy season drilling and firing became both intermittent and dangerous, apprehension was felt that periods might consequently occur when there would be a shortage of stone. For that reason a scheme for making a "tunnel" blast, using about 10 tons of explosive (60 per cent. gelignite) was considered. It seemed generally advantageous to do that, but it was thought that, since the quarry was situated close to the dam, the effects of the blast might induce cracks in the tunnel lining, the tongue-trench concrete, or might open up a joint or fissure in the rock below it. It was necessary to be sure that no damage of that kind would occur. An experiment was therefore carried out which would, it was hoped, give enough information about the intensity and frequency of waves through rock, caused by the released gases, to show whether or not the big blast could be made safely. After considering several possible methods of acquiring the necessary information it was decided to follow the lead of Mr. Rockwell, an American engineer who had devoted a good deal of attention to the effect of blasts. Steel pins of equal diameter, but of different lengths, were set up at various distances from the centre of the

experimental explosion. The force required to rotate a pin could readily be ascertained, and by noting which pins fell and which remained standing after the blast, the length over which the kinetic energy of the motion of the vibration was effective, could be found.

A section of quarry, as far as possible a replica of the part proposed for the big blast, was selected and three vertical holes 15 feet deep were drilled 9 feet apart, each being charged with 13 lb. of explosive (60 per cent. gelignite), the total charge of 39 lb. being proportional to the charge estimated to be necessary for the tunnel blast.

The pins used were $\frac{1}{2}$ inch in diameter and 4 inches, 11 inches, and 18 inches in height with true ends. One of each length was set up in each of 8 positions ranging from 20 to 87 feet from the centre hole. The three holes were fired simultaneously and the effect just reached the rods 38 feet away; those beyond that distance remained standing. That fact, mathematically translated into terms of a 20,000 lb. shot, led to the belief that all parts of the work further than 304 feet from the centre of the blast would be completely unaffected by it. As the nearest part of the tunnel lining was 720 feet away and the thrust block 1,150 feet, it was decided to proceed.

Useful and comparatively accurate information regarding the amount of explosive required to bring down a ton of rock was, by that time, available from the quarry records. Using that as a basic figure and allowing for the known better effect of a concentrated charge, and anticipating also a break back of 30 degrees from the ends and back of the T-head tunnel, and bearing in mind that the adjacent plant was not to be damaged by widely flung pieces of rock, it was decided to use 1 lb. of gelignite to 8 tons of rock and to aim at breaking the hill up gently into large pieces, which could be broken up by secondary drilling or by jack-hammer.

The length of the approach tunnel was 120 feet and of the T-head, 145 feet, the latter being driven parallel to the face of the quarry; both were 6 feet by 4 feet in section. At an equal distance apart five pits, 6 feet square and 3 feet deep, were sunk along the back tunnel and each was loaded with 4,000 lb. of gelignite in boxes. The tunnels were carefully and tightly back-filled with quarry rubbish after the electrical connexions to the five primers had been made.

Before firing the shot, cement tell-tales were placed across some of the joints in the rock in the unlined part of the tunnel and $\frac{1}{2}$ -inch-diameter pins, 18 inches and 11 inches in height, were set up at various places in the diversion tunnel and on the dam. Water pressure was maintained on the holes drilled for grouting in the tongue trench. The charge had the effect of gently lifting the hill and breaking it up into masses, the largest of which weighed about 400 tons. The total amount of usable stone produced was estimated at the time to be 180,000 tons. That estimate proved to be nearly right by the records which were kept during the 5 or 6 months it took to break up the blasted rock to rock-fill size. Thus the resulting figure

for explosive effect was about 9 tons to the lb. None of the cement tell-tales opened up, there was no drop in pressure of the water in the grout holes, and all pins remained standing except two at the near end of the tunnel; thus the fact was established that, in granite formation, a charge of approximately 10 tons of explosive could safely be fired if its centre were more than 1,000 feet away from a partially completed dam.

The blast, as had been desired, provided a supply of stone for 6 months, the duration of the wet weather. Mr. Hull made it clear that a mass of that size and shape was not so conveniently worked for secondary blasting and removal as a mass produced by 30 or 40 holes in line: nevertheless, the cost of rock-fill stone from the tunnel blast was lower than that produced by ordinary methods. A similar blast could not be made again, however, as the quarry had to be worked in three levels 100 feet apart vertically, the quarry plant, rails, locomotives, travelling cranes, and flat cars being successively lifted to a higher level as the height of the dam was increased. The formation at the upper levels did not lend itself to a tunnel blast. Apart from that, rock would have been lost by falling to the original quarry floor, from which the plant to handle it had been removed.

References to cost had been made in the Paper. It was difficult to compare accurately the costs of work in one country with those of similar work in another, but nevertheless it could be done, and the information so obtained could be useful to engineers concerned with similar works. Often it was thought that in a country where labour was cheap, costs would inevitably be lower than in a country where labour was expensive. But that was not necessarily so. There were other factors which had as great an influence on cost as labour, one of which was plant lay-out. There were, for instance, the measuring and handling of concrete aggregates and cement, mixing the concrete, transporting it to the dam by cableway, placing it in 6-inch layers, ramming it with air rammers, including the making of rough shuttering to divide it into appropriate blocks and all power. The total cost of the above items never exceeded 50 cents. (8*d.*) per cubic yard. That was not due to cheap labour. If, instead of receiving \$1.00 per day, the men controlling the mixer battery had received 10*s.* per day (8 times as much), the corresponding part of the above costs would have been 12 cents, or about 2*d.* per cubic yard, and it would be conceded that that was a really low figure in any circumstances anywhere. That economy was achieved by designing the plant so that it would require the minimum number of operatives rather than by relying on the cheapness of manual labour. Similarly the arrangements for transportation and placing were designed with the object of reducing the number of workers to an absolute minimum. If, on the other hand, labour had been largely employed because it was cheap, in preference to a deep concentration on the design of plant, which was expensive, it was clear that, on such a large job, many hundreds more men would

have been required. That would have meant more staff to manage them, more bungalows and coolie lines to house them, more water-supply, lighting, sanitation and medical attention, and more timekeepers and clerks. The resulting total cost of the work would have been higher, not lower.

With that point in mind, it might be of interest to give the costs of some of the major features of the work. The costs given below were for labour, materials, and power : they were not selected costs for any especially good month, but were the average costs of the gross final quantities.

	Cost per cubic yard :		Equivalent in sterling :		Approximate quantity :
	\$	c.	s.	d.	
Excavation for main foundations, in earth.	0	82.54	1	1½	21,065
Excavation for main foundations, in rock	2	62.42	3	6	13,907
Excavation for cut-off trench up to 70 feet below surface, in rock	5	17.34	6	10	8,964
Class A concrete in thrust block (300 lb. cement per cubic yard)	6	88.44	9	2	131,602
Class B concrete in cut-off (600 lb. cement per cubic yard)	10	28.20	13	8	19,894
Class C reinforced concrete, in diaphragm (690 lb. cement per cubic yard), excluding cost of reinforcement	13	79.00	18	5	8,893
Closely packed rock-fill per cubic yard .	2	94.78	3	11	480,598

The costs shown included for material, labour, and power. The overhead charges on the Shing Mun work were known as Local General Expenditure and included camp-maintenance, water-supply, lighting, sanitation, police, general office expenses, medical attention, anti-malarial works and maintenance, transport, surveys, typhoon damage, office and staff salaries, passages, and handling of stores. Those charges amounted to 27.55 per cent. of the net costs given above. Plant and tools charges amounted to 21.66 per cent. Total overheads therefore amounted to 49.21 per cent. and the net costs given above should be increased by that percentage to give the final cost. The total percentage for on-costs would appear to be high, and in order to present as true a picture as possible, it might be stated that anti-malarial works involved the laying of 22 miles of concrete sub-soil pipes and surface channels covering a treated area of 986 acres, at a cost which represented 2.29 per cent. Medical attention absorbed 1.11 per cent., and the transport of plant, stores, and materials over a distance of 11 miles, 0.95 per cent.

Although the full cost of plant, amounting to \$774,290, was charged to plant and tools, and was included in the 21.66 per cent. given above, much of it was sold upon completion of the work and its salvage value was likely

to realize 50 per cent. of its cost. That would reduce the plant and tools percentage by 7·2 per cent. to 14·46 per cent.

Staff salaries and passages amounted to 16·62 per cent., which was more than half of the Local General Expenditure.

The Hong Kong dollar fluctuated widely for a short period, but as the plant and materials from outside had been bought and paid for before the dollar reached its high level, the fluctuation did not materially affect the work. The average rate of exchange was about 1s. 5d. over the whole period of construction.

Mr. Hull thought that it might not be out of place, when referring to costs, to describe briefly the purchase and handling of cement. At the time the works were started, it was possible to buy good-quality Japanese cement at a price per ton which would save nearly \$1,000,000 on the cost of cement supplied by the local cement works. Opinion was divided in Hong Kong as to whether the above sum should be saved by the purchase of Japanese cement (a course to which, for other reasons, the engineers were reluctant to consent), or if it should be spent on helping the local works, whose market in Singapore and South China had been seriously affected by Japanese cement and cement from the newly-erected works in Canton respectively. His Excellency the Governor, Sir William Peel, ordered a close study to be made of the matter, to be followed by a Report. That study showed that, if it were possible to use cement in bulk, transport and handling costs would be considerably less, there would be less waste, no cement shed would be required, and it would be fair to press the local cement company for a lesser price per ton, since bags would not be required and its own handling costs could be reduced. Very considerable attention was given to the design and cost of all stages in the works-to-site cycle. The rather surprising result of the study was that the cost of the local cement, at its final place in the mixers, could be reduced to within 20 per cent. of the cost of the Japanese cement. The Government considered that the extra cost was, in the special circumstances, justified, and the bulk cement scheme was approved. Later, as mentioned in the Paper, the engineers decided that the dam would have to be built below the waterfall, and the new design prepared for that site required considerably less cement. The final cost-difference thus became of less moment.

The broad proposal was to transport the cement in barges containing twenty-four steel containers, each weighing 4 tons when full, to a wharf which had been built at Tsun-Wan, a point on the coast about 4 miles from the works, to unload them there by derrick, and to convey them to the works by 6-ton lorries. At the works the cement was to be dumped into a 40-ton silo above the mixers and used by weight. It was necessary first to design the containers so that all the cement would flow easily from them through a door at the mouth which would be waterproof and which, at the same time, could be easily opened. After several attempts to design such a door, all of them lacking perfection under test, the problem was solved

by Mr. Mettham, the manager of a local dock company, who designed a most ingenious and perfectly satisfactory door. The containers were then designed so that the standard local 100-ton lighter could take twenty-four of them in two rows of twelve, standing vertically so that they could be loaded conveniently. It was necessary to devise a system under which the containers could be loaded at the works without being removed from the lighters. That was done by conveying the loose cement from the works' silos to the lighters on a covered-in belt which delivered it into a metal box containing a flap-valve. That flap-valve could divert the cement into either of two canvas shoots, 9 inches in diameter, suspended over the lighter. The tidal range in Hong Kong varied according to the season from 4 to 8 feet and it was therefore necessary to allow for that, and to provide for loading at any stage of the tide. It was necessary to have more than one shoot, otherwise the belt conveyor would have to be stopped while the shoot was being changed from a filled container to an empty one.

The lighters were towed by tugs to the jetty at Tsun-Wan. There the containers were lifted by derrick into their vertical position, and, with a specially designed sling, turned to a horizontal position and laid in cradles on the lorries for transport. At the works they were once again handled by derrick, turned mouth downwards over the port in the roof of the silo, emptied and returned to the lorry.

The slope of the shoots from the bottom of the silo was such that the cement could flow easily into a weighing hopper for each mixer, fixed at the end of a steel-yard type scale. By placing weights, equal to the weight of cement required for any particular class of concrete, at the opposite end of the yard-arm, a correct and unvarying amount of cement could be added to each mix. As the weights were lifted by the weight of the cement in the hopper, the cement ceased to flow and, almost simultaneously, the gate to the mixer hoppers was opened, allowing the cement to flow into them, an electric bell sounding when the aggregate was ready for cement. Accurate mixes were thus prepared in a few seconds with a minimum of labour and at very low cost. The concrete was carried to the dam by a 5-ton cableway in skips of 2 cubic yards' capacity. It was not convenient or practicable to have more than one cableway across the dam and that one had to be of the fixed-tower type. Therefore concrete could only be delivered at points along the line of the main cable. To avoid the comparatively costly hand-spreading over the surface of the thrust block, a distributing tower was designed. That was of light steel construction 25 feet high and carried at its top a steel hopper of 2 cubic yards' capacity. The hopper bottom was so formed that concrete could flow easily to five openings, one at each side and one at the centre. Loose metal shoots in 12-foot sections, each 10 inches diameter with a bell-mouth top, were hung by chains from the hopper under the openings and the shoots themselves were loosely held together by short chains, to give complete flexibility. The complete tower was designed so that its weight was within the lifting

capacity of the cableway, and it was set up by the cableway on concrete piers, 12 feet high, which had been previously built on the dam. The upper shoots were held at an angle of 35 degrees from the tower by steel arms. The tower could distribute concrete to any point over an area of approximately 2,000 square feet with one man moving the bottom section of the shoots. Trials were conducted to find the best diameter of shoot. Too large a diameter allowed the concrete to segregate, whilst if the diameter were too small, the concrete clogged in the shoot. A diameter of 10 inches eliminated both those difficulties. When the concrete-level reached the height of the piers on which the tower was set, the tower was picked up bodily by the cableway and set in a new prepared position in a few minutes. The tower was successful and spread many thousands of cubic yards at a very low cost.

As the dam increased in height it became longer and narrower. The longer carry naturally took a longer time and the number of trips made by the cableway was reduced by 20 per cent. In order to maintain the mixers at their full capacity it was arranged to use the tower on the further or left flank of the dam and to arrange a shoot from the mixers for the near flank, delivering into wagons which dumped the concrete either side of a raised track. Much attention was given to the design of the shoots by Mr. J. C. Campbell, the construction engineer on the dam, with a view to the prevention of segregation, and to other defects of steel shoots in hot countries, such as sticking. It was found that a smooth flow with no segregation was given by an angle of 35 degrees, but over a longer length than 40 feet the concrete was apt to stick. The shoot therefore was divided into sections, all except the bottom one being fixed at an angle steeper than 35 degrees in order to avoid sticking. Minor segregation took place in the upper shoots but by introducing remixing falls or checks at the end of each length of shoot the faster-moving particles were thrown back into the mass and the concrete virtually remixed until it entered the bottom section at 35 degrees, down which it was carried into position in a perfectly-mixed condition.

One interesting feature of the thrust-block concrete was that, although it was placed rapidly, and no special arrangements, such as the refrigerating system of the Boulder dam, were made to reduce the generation of heat, repeated examinations disclosed no cracks after setting. That was thought to be partly due to the fact that the cement company were able to meet the engineers' wishes to supply a coarser ground low-heat cement. The average rise of temperature in the thrust block was about 24° F. in 40 hours. Unfortunately, no large-scale practical tests could be made with cement of normal fineness, but the probability was that it would have given a higher temperature-rise and, consequently, a greater susceptibility to cracking.

Mr. Hull noted that special attention was given to the curing of concrete, particularly that forming the face of the dam. The instructions given by the engineers were that the whole of the surface should be kept

wet for a minimum period of 3 weeks, and as long as possible thereafter. To that end, a perforated water-pipe was carried over the top of the panels with a small metal disk over each hole which had the effect of breaking up the issuing water into a spray, thus effectively damping the surface of a panel. Except during the raising of the pipe following the placing of a higher lift of concrete, the wetting was continuous over the entire surface of the face of the dam.

Mr. Kenneth B. Keener, of Denver, thought that one of the most interesting of the many distinctive features of the Gorge dam was the slip joint between the face concrete and the thrust-block, which was constructed in recognition of the difference in temperature-rise of the two concrete mixtures. If that joint had not been formed, doubtless an irregular rough crack, along that same location, would have resulted. Such a crack would not have permitted differential settlement except by crushing of the projections or irregularities, thus producing a comparatively large zone, variable in width, of inferior concrete. That might possibly have been cured in time by autogenous healing, but it would always have been a probable plane of weakness and he agreed that a formed joint was the proper procedure.

Mr. Keener believed that the potential cracking across, or through, the rich face concrete would have been no greater had the slip joint not been formed. Such cracking might be caused by variation in temperature throughout the face block, and possibly by differential settlement of the foundations.

The amount of leakage water since impounding was remarkably low. That demonstrated not only that the various design details to prevent leakage were effective, but also that the construction had been carried out with the same thoroughness. It happened too frequently that the purposes of adequate designs were defeated by inferior construction.

That unusual precautions had been taken to prevent leakage was evidenced by the installation of copper strips across the horizontal construction-joints. The problem of the proper preparation of those joints was one that was receiving much consideration, even in concrete gravity dams. It would seem that a satisfactory solution could be found without recourse to water-stops. **Mr. Keener** knew of two massive concrete dams now under construction in the United States where such stops were being installed across the joints, near the upstream face. That detail, however, had not been adopted by the Bureau of Reclamation.

Mr. T. G. Owen, of Denver, who was conducting model-experiments on siphons similar to those of the Gorge dam, thought that a comparison between the results obtained by two investigators, working independently, would be of interest and might add to the information presented in the Paper. To render the comparison more intelligible, he would give a brief description of the siphons on which he was working.

Those siphons were to be used on wasteway No. 2, Roza division,

Yakima project, Washington, U.S.A. They were being tested by an $\frac{1}{8}$ -scale model in the hydraulic laboratory of the United States Department of the Interior, Bureau of Reclamation, Denver, Colorado. Those siphons would be built at the same time as the wasteway structure, although they were not expected to operate until after a contemplated power-plant and appurtenant works had been constructed. They would then operate as automatic emergency spillways and would prevent water from rising above the top of the lining in the forebay or feeder canal whenever the supply to the turbines was suddenly cut off.

There were to be four siphons with a combined maximum discharge of 950 cusecs. Two would be located on either side of the wasteway structure, which was to be equipped with non-automatic radial gates. These siphons would discharge into the wasteway channel at a point about 30 feet downstream from the radial gates. Table I showed a comparison of the general dimensions of the two types of siphon and indicated their similarity.

TABLE I.

Item.	Gorge dam siphons.	Wasteway No. 2 siphons.
Radius of crest: feet	6	4
Length of crest: feet	6	6
Depth at throat: feet	4	2-167
Area of throat: square feet	24	13
Ratio of centre-line radius to throat depth (R/D)	2-0	2-35
Approximate operating suction head: feet.	21	10-5
Theoretical maximum discharge: cusecs	675	407

Both types of siphons had an S-shape, a rather large radius at the lower bend, and a diverging exit tube. The entrances to the siphons were, however, in no way similar. The lip of the hood of the Gorge dam siphons was submerged 8 feet below the crest, whilst the lip of the hood in the wasteway No. 2 siphons was placed at the normal forebay elevation.

The method of computing the efficiency of the wasteway No. 2 siphons was the same as that used by the Authors, so a comparison between the efficiencies of the two siphons might be made directly. The formula for efficiency was

$$n = \frac{Q}{A\sqrt{2ga}},$$

where Q denoted the discharge of the siphon in cusecs,

A denoted the area of the throat section in square feet,

a denoted the atmospheric pressure in feet of water.

At sea-level $a = 34$ feet and the efficiency became

$$n = \frac{Q}{47A}.$$

In computing the maximum attainable efficiency of the Gorge dam siphons, a permissible vacuum, at the crest, of 21 feet, or about 70 per cent. of the lowest recorded barometric pressure at the location, was assumed. That gave a crest velocity of 36.8 feet per second. The maximum permissible discharge per foot of crest was expressed by :

$$Q_1 = \int_{r_0}^R V dr = 36.8 r_0 \log_e \frac{R}{r_0},$$

where r_0 denoted the crest radius,
and R denoted the radius of the crown.

The maximum attainable efficiency became :

$$n = \frac{Q_1}{47(R - r_0)} = 0.78 \frac{r_0}{R - r_0} \cdot \log_e \frac{R}{r_0} = 0.6, \text{ or } 60 \text{ per cent.}$$

The throat area of the Gorge dam siphons was 24 square feet ; hence, the permissible discharge became 675 cusecs and the corresponding coefficient of discharge based on the throat area became 0.76.

In the siphons of the wasteway No. 2, the maximum permissible vacuum was assumed to be 24 feet, or about 75 per cent. of the atmospheric pressure at the elevation of their location. By the same method as used in the investigations on the Gorge dam, the maximum attainable efficiency was found to be 67 per cent. and the maximum discharge 407 cusecs. Also, basing computations on the outlet area and on an effective head of 8.43 feet, the corresponding coefficient of discharge was found to be 0.73.

The siphon finally adopted for the Gorge dam was essentially the same as the model called type No. 3, except that type No. 3 had a crest radius corresponding to 8 feet whilst the siphon adopted had a crest radius of 6 feet. Model-tests on type No. 3 indicated a coefficient of discharge of 0.70, based on the throat area ; the time to prime was 170 seconds when the rate of rise of the forebay elevation was 0.0036 foot per second (model). The head at which the siphon primed was 0.615 foot (model). The so-called " priming efficiency " expressed the minimum head required for priming in the terms of the depth of the barrel at the throat section. Thus in the Gorge dam siphons where the depth at the throat was 4 feet and the head required for priming was 0.615 foot, the priming efficiency was $\frac{d}{1.63}$.

The recommended design of the siphons for wasteway No. 2 was shown in *Fig. 23* (p. 440). It incorporated the lip shown in *Fig. 24* (p. 441) and the priming device shown in *Figs. 25* and *26* (pp. 442-443). The siphon efficiency indicated by the model-tests was 52 per cent. ; the coefficient of discharge based on the outlet area was 0.57 ; the minimum head required to prime was 0.019 foot (model), giving a priming efficiency of $\frac{d}{14.45}$; and the time required to prime at that head was 59 seconds (model).

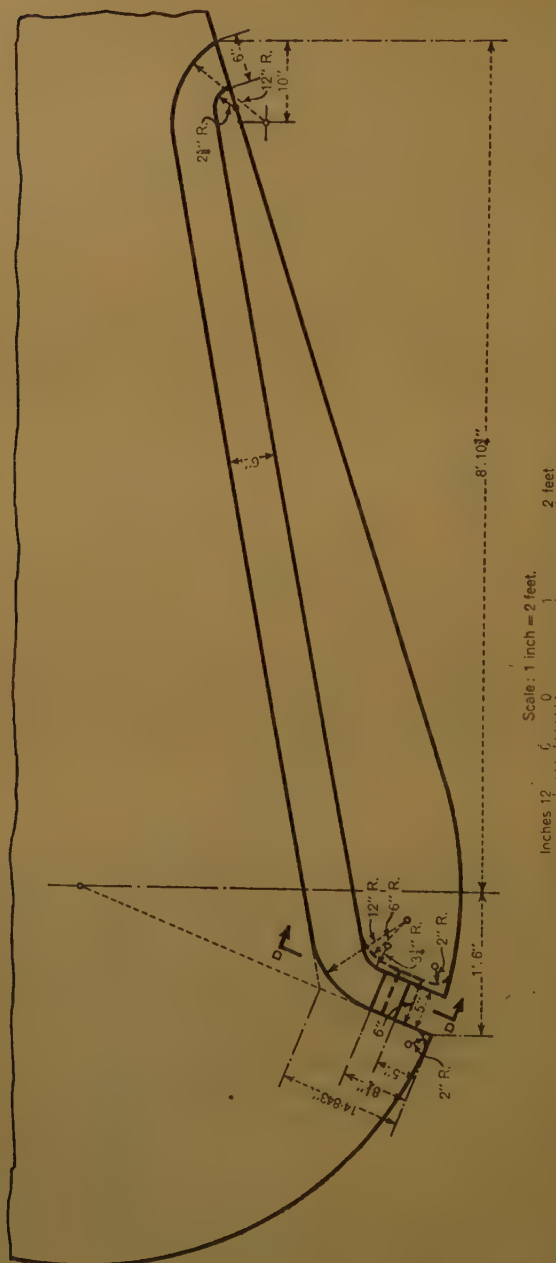
or to less than one-third of the value found when no seal was used. The reason for that difference was: the jet, springing free from the crest and striking the far wall of the barrel, produced a curtain of water, which sealed the upper part of the siphon from the lower; the upper surface of the jet gradually evacuated air from the upper part of the siphon until there existed a difference of pressure between the parts above and below the curtain-seal; then, when that difference of pressure reached a certain value, the jet, as observed through the transparent walls of the model,

Fig. 24.



assumed a fluttering motion, which increased in intensity until the curtain seal was completely broken, and the pressure between the upper and lower parts was equalized. That led to the assumption that the minimum head required for priming depended on the thickness of the water-curtain, and that the siphon would prime only after the depth of overflow became sufficient to establish a curtain-seal of such thickness that it could not be broken by the pressure difference. Furthermore, it was concluded that the siphon would prime at a much lower head if a water-seal were provided at the lower bend. Those assumptions were verified by tests using a water-seal. Experiments to increase further the priming efficiency were carried out. Various devices were tried in the model. The best combination incorporated the lip shown in Fig. 24, and the priming slot shown dotted in Fig. 25 and in section in Fig. 26. Incorporation of those devices reduced the minimum priming head to 0.15 foot (prototype) and gave a priming efficiency of $\frac{d}{14.45}$.

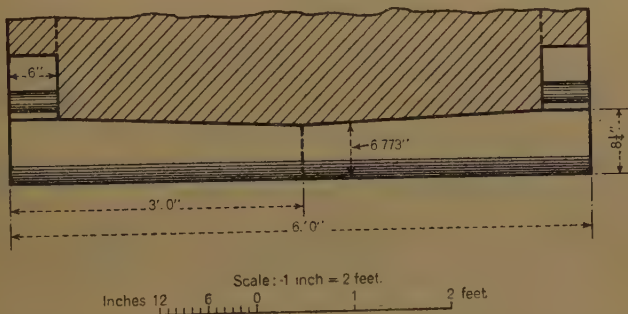
Fig. 25.



PRIMING DEVICE FOR SIPHONS: WASTEWAY NO. 2.

At the Gorge dam the minimum head required for priming was defined as that head at which the siphon primed when the forebay water-level was rising at a constant rate. The model-experiments indicated a priming efficiency of $\frac{d}{1.63}$. In wasteway No. 2, however, the minimum head required to prime was determined by holding the forebay water-level constant. The recommended design indicated a priming efficiency of $\frac{d}{14.45}$. The difference in priming efficiency between the Gorge dam siphons and those of wasteway No. 2 was due in part to the difference in definition of minimum head to prime, and in part to the priming devices and water-seal at the lower bend, which were used in the wasteway No. 2 siphons. In

Fig. 26.



SECTION DD IN Fig. 25.

cases where freeboard was abundant, as at the Gorge dam, a high priming efficiency was not important; where, however, the available freeboard was limited, as in the case of the wasteway No. 2 siphons, a high priming efficiency was imperative. Generally speaking, a high priming efficiency was obtained at the expense of the siphon efficiency.

The computed maximum attainable efficiency of the Gorge dam siphons was 60 per cent.; the efficiency indicated by model-tests, with a head at the crest of 2 feet (prototype), was 59.75 per cent. The computed maximum attainable efficiency of the wasteway No. 2 siphons was 67 per cent.; model-tests, with a head at the crest of 0.88 foot (prototype), indicated an efficiency of 52 per cent. for the siphon incorporating the features shown in Figs. 24, 25, and 26. Mr. Owen pointed out that the reason the experimental efficiency did not more closely approach the computed maximum attainable efficiency for the wasteway No. 2 siphons was that the maximum permissible vacuum was 24 feet, whilst the available suction head was only about 10.5 feet. In the Gorge dam siphons, on the other hand, the maximum permissible vacuum was 21 feet and the available suction head also was 21 feet. He based his observations on the assumption that the efficiency of a given siphon was the ratio of the actual discharge to the

theoretical discharge that would be obtained if a perfect vacuum could be established. A better method would be to define the efficiency as the ratio of unit discharge to unit cost. Unfortunately, comparative figures on that basis were not available.

The Gorge dam experiments revealed a coefficient of discharge of 0.70 based on the throat area, as compared with the theoretical permissible coefficient of 0.76. If an efficiency of design could be considered as the ratio of the actual to the theoretical coefficient, the Gorge dam siphons had a "design efficiency" of 92 per cent. Experiments on the wasteway No. 2 siphons, already described, indicated a coefficient of discharge of 0.57, based on the outlet area, as compared with the theoretical permissible value of 0.73. Thus the "design efficiency" was 78 per cent. The experimental coefficient of discharge for the wasteway No. 2 siphons, based on the throat area, was 1.06; the permissible coefficient, based on the throat area, was 0.80. That would give a "design efficiency" of 133 per cent. From the foregoing it might be assumed that the permissible vacuum at the crest, and the corresponding permissible discharge, had been exceeded; yet, the allowable discharge was 407 cusecs., whilst the experimentally determined value was 320 cusecs. In the design of a siphon the permissible discharge, or coefficient of discharge, should be determined, based on the permissible vacuum at the throat, as had been done in the case of the Gorge dam siphons. After a comparison of the experimental coefficient of discharge had been made with the permissible coefficient to determine whether or not the allowable vacuum at the crest had been exceeded, the coefficient of discharge had no further function.

A study of the results of the Gorge dam siphons and of those of wasteway No. 2 pointed to the conclusion that in well-designed siphons the actual performance would closely approach the theoretical. Furthermore, the performance characteristics of siphons of similar shape were similar to a remarkable degree.

Mr. E. G. Ritchie, of Melbourne, drew attention to the novelty of the design and thought that it constituted a definite contribution to the sum of engineering knowledge concerning high-dam construction. The special considerations regarding earth tremors called for an elastic type of dam, and an outstanding feature of the Gorge dam was the remarkable stability attained in the rock-fill portion of the structure. Doubtless that was due to the meticulous care with which the manual labour must have been supervised. A weight of rock-fill per unit of volume equal to two-thirds that of solid granite could only have been secured by close supervision over hand-packed work. It was only by reason of the cheapness of Chinese labour that so much hand work was economically possible, and Mr. Ritchie doubted whether such construction would be economical in countries where labour conditions were not so favourable. It would, therefore, greatly add to the value of the Paper if the Authors could give particulars of the cost of the rock-fill in place. If the unit costs could be

given separately for quarrying, transport, and placing, the information would be still more valuable. Furthermore, if the Authors could relate such costs to those which would obtain in England (wages and hours of work being quoted), engineers could form a better opinion of the applicability to other situations.

The deflexion of 0.57 inch in the upper part of the thrust-block was a remarkably good result, and it would be of interest to know if, and to what extent, such deflexion was likely to be progressive over the next 5 or 10 years. Such information would be a further most valuable contribution to engineering literature.

Mr. Ritchie compared the arrangement of buttresses and inspection pits with the form of cellular construction which had been incorporated in the concrete core-wall of the Silvan dam, Melbourne water-supply, Australia.

The Authors had furnished information (p. 186 §) concerning the boreholes, which were tested to a pressure of 100-lb. per square inch. At what distance apart were those boreholes drilled? Any information relating to that would be useful.

Mr. Ritchie commented on the employment of steel in the standpipe and outlet pipes. It was not clear whether those could be easily repaired or replaced without seriously interfering with the output of water, but perhaps the Authors could explain that. It would, furthermore, be interesting to know whether cement lining was used, and if not, what internal protection to the steel was adopted. Would the Authors state the considerations which influenced the choice of steel, in preference to a more permanent form of construction such as cast iron, in that vital part of such a fine work?

Mr. C. M. Saville, of Connecticut, observed that the structure belonged to the category of "high" dams, and was of such unique design as to arouse interest among engineers familiar with that class of construction. The outstanding achievement in the design of the Gorge dam was the unique methods by which all the usual complicated features of high-dam design had been adapted to that particular locality which offered many obstacles to stereotyped construction in method, type, and materials.

Mr. Saville approved of the special attention given to making the concrete masonry impermeable, both in proportioning and in methods of placing, whilst the greased paper between the two kinds of concrete, to prevent adverse action of temperature-stresses in two different mixes of concrete, if joined together, seemed a wise precaution and much more effective than the method, often used, of covering one side of the concrete block with an asphaltic paint. Every precaution seemed to have been taken to ensure tight foundation conditions, using excavation to sound rock, and grouting below where there was evidence of cracks and seams. The elimination of contraction- or expansion-joints in the face concrete was

a departure from ordinary practice, but such joints were usually a source of annoyance, and in the present case such omission seemed justified.

Mr. Saville referred to *Fig. 8* (p. 193 §), and observed that he would be interested to know if climatic and local conditions might be expected, in time, to have any deleterious effect on the exposed concrete in the face of the dam or in the side walls on top. In his experience in the north-eastern United States there were conditions more or less injurious to appearance, if not integrity, of concrete where exposed to climatic changes, regardless of the mixture and pains taken in fabrication.

Mr. Jacob E. Warnock, of Denver, observed that the treatment of the flood-disposal works in the design of the Gorge dam was of particular interest, in that it offered an additional example of the complexity of providing adequate flood-flow capacity at a reasonable cost. The solution of the problem created by model-tests illustrated the practically universal application of that type of analysis.

Recent progress in model-analysis, in the solution of hydraulic problems, had been phenomenal, and had resulted in designs that offered more assurance of correct behaviour with less so-called "factor of safety." In many cases those designs had led to more economical construction as a result of that greater assurance.

The character of the terrain at the Gorge dam site had led to the adoption of the circular bellmouth weir. Another type of overflow which might have been economically adapted to that site was the side-channel spillway such as was designed by the Bureau of Reclamation to control the flood waters at Boulder dam, and, on a smaller scale, at the Grassy Lake dam on the Upper Snake River project in the state of Wyoming. The side-channel spillway was particularly advantageous in steep canyons such as prevailed at Gorge dam. A side channel with a crest length of 210 feet and a 15-foot-diameter tunnel would have a capacity of approximately 18,000 cubic feet per second with a head of 8 feet. Increased capacity to handle a large flood flow could be obtained by the enlargement of a short section of the tunnel below the spillway and by a slight increase in the freeboard.

The indraught of large quantities of air in a structure such as the bellmouth weir and tunnel was a phenomenon which required considerably more study. Whilst the simultaneous flow of air above high-velocity water had always existed, the advent, during the last two decades, of structures of unprecedented magnitude, had increased the significance of that action. Very little information existed, because of the difficulty of obtaining it; it was not a property capable of analysis in a model because of the change in characteristics of the stream surface in the prototype as compared with that in the model. The surface of a high-velocity stream, in even a large-scale model, was relatively smooth, whereas in the prototype

a condition was quickly reached at which the water surface became turbulent. That turbulence caused the absorption of large quantities of air in the water and probably caused considerable change in the flow characteristics. In an open channel with ample freeboard, that increase in cross-sectional area and probable reduction of velocity of the stream might be of little consequence, but in a closed conduit the change might cause belching at the inlet. The lack of information of that condition was due to lack of structures of sufficient height. Even if structures had been available, it was doubtful if any large amount of data could have been collected due to the intermittent operation and the extreme conditions under which the data would have to be obtained.

The Authors, in reply, observed that the movements of the lower portion of the dam under water load, which were shown in *Fig. 10* (p. 196 §), were referred to by Mr. Houk, who attributed the upstream deflexion to the elastic deformation of the granite immediately above the dam under water load, citing as an example the movements which had taken place at the Norris dam. The upstream tilt at the Norris dam was, however, partially attributed to the difference of temperature of the upstream and downstream faces, which could not be an important factor at the Gorge dam as the downstream face was "shaded" by the rockfill and the difference between water- and shade-temperature was unimportant. The relationship between water-pressure and upstream tilt of the dam was borne out by experience during the last year, and the Authors were of opinion that the explanation given by Mr. Houk was probably correct.

The movements of the upper portion of the dam in a downstream direction under water load were referred to by Mr. Ritchie, who stated that it would be of interest to know whether the deflexion was likely to be progressive over the next 5 or 10 years. The reservoir overflowed in September 1937 and in October the maximum deflexion amounted to 0.57 inch downstream, overflow-level being 625 O.D. The monsoon of 1938 was poor, and the water-level did not rise above 594 O.D., or 31 feet below overflow-level, the corresponding deflexion amounting to 0.16 inch. Information received from Hong Kong dated the 30th June, 1939, gave the water-level as 622.25 O.D. or 2.75 feet below overflow-level, and the deflexion downstream 0.47 inch. It was expected that "creep" would take place and that the "deflexion" would be progressive, but so far the movements appeared to be entirely elastic.

The Paper did not deal with construction methods or costs as the Authors were desirous that Mr. G. B. Gifford Hull, M. Inst. C.E., to whose skill and care the success of the work was to be largely attributed, should do so, and they were greatly obliged to him for his contribution.

Reference was made by Mr. K. B. Keener to the method adopted to prevent percolation through horizontal construction-joints by means of

vertical copper strips intercepting such percolation, and the opinion was expressed that a satisfactory solution could be found without resort to such water-stops. The Authors agreed with that opinion provided that perfect workmanship could be relied upon, but an examination of a number of masonry dams revealed that percolation was taking place along those joints, and it was decided to take steps at the Gorge dam to prevent all risk of such percolation occurring.

The design of the siphons was dealt with by Mr. T. G. Owen and the Authors were greatly indebted to him for the information he had given, which was particularly valuable when it was essential that the siphons should cease to discharge under a low "priming" head. Reference was made to the fact that, whereas the lip of the hood of the Gorge dam siphons was submerged 8 feet below the level of the crest, the lip of the hood in the wasteway No. 2 siphons was placed at the normal forebay-level. The Authors deemed it advisable to submerge the lip to a depth of 8 feet in order to minimize "gulping" due to oscillation of water-level in the reservoir, and probable formation of major vortexes, which might permit the access of large volumes of air to the siphons.

Mr. Ritchie commented on the density of the rockfill, which could only be economically attained when labour was cheap, and asked for the cost; that had been given by Mr. Hull.

Mr. Ritchie raised a question with regard to the spacing of the boreholes. The following account of the method adopted for securing a water-tight foundation might be of interest as typical of foundations in slightly fissured granite.

The trench was evacuated into the granite until approved by the engineers for testing. 2-inch test holes, spaced 15 feet apart, were then sunk to a depth of 20 feet below the bottom, and subjected to a water test under a pressure of 100 lb. per square inch. If the leakage under that pressure amounted to more than 1.3 gallon per minute, the trench was to be deepened or intermediate holes were to be drilled for grouting. That test revealed that there were ten places requiring further attention, the trench being deepened at four of those places and intermediate holes drilled at the other six. Seventy-three test holes were sunk in all, and grouted to refusal with grout containing 1 part of cement to 12 parts of water by volume, followed up by thicker grout in some instances, the pressure applied for grouting being generally 250 lb. per square inch. One hole absorbed 1,355 lb. of cement, the average quantity taken by the others being 66 lb. Where joints were visible in the trench, further grouting pipes were inserted and brought up with the concrete filling for subsequent high-pressure grouting with 12:1 mixture, those pipes being fifty-five in number. The average quantity of cement absorbed was 70 lb., with the exception of one pipe, which took 1,530 lb. The average spacing of the test holes and grout pipes was about 8 feet, but it varied considerably according to the nature of the foundation rock. It was

pointed out on p. 187 § that the maximum amount of leakage below or through the tongue-trench concrete was 430 gallons per hour, which was considered satisfactory.

With regard to the use of steel for the standpipe in the valve-tower in preference to cast iron, the pipes had an internal diameter of 48 inches, the wall being $\frac{1}{2}$ -inch thick, protected from external corrosion by means of reinforced cement mortar. Steel was preferred to cast iron both on account of weight and greater reliability. The steel pipes could be replaced, if necessary, without interfering with the supply of water to Kowloon and Hong Kong.

In reply to the point raised by Mr. Saville, it was not expected that the climate would have any deleterious effect on the exposed concrete, all of which was of a very dense character. Such deterioration had not been noticed in the British Isles, where the climatic conditions were more severe than at Hong Kong.

Mr. Warnock advocated the side-channel spillway in preference to the circular bellmouth overflow, and the Authors agreed that the former was often more economical. The mountain slopes were so steep at the Gorge dam that a very large amount of material would have had to be removed to form a benching of any considerable extent on which to found a side-channel spillway. Fig. 16, Plate 2 (facing p. 222 §), showed that the overflow was supported by the conical shaft for about half of its periphery, thus minimizing excavation necessary to allow access of the water to all parts of the overflow weir.

In conclusion, the Authors much appreciated the remarks by Mr. Ritchie and Mr. Saville drawing attention to the design as being unique and constituting a definite contribution to the sum of engineering knowledge concerning high-dam construction.

§ *Ibid.*

Papers Nos. 5158 and 5201.

“Some Experiments on the Lateral Oscillation of
Railway Vehicles.” †

By RALFE DAVIDSON DAVIES, M.A., Ph.D., Assoc. M. Inst. C.E.,
and

“The Vertical Path of a Wheel Moving Along a
Railway Track.” ††

By Professor CHARLES EDWARD INGLIS, O.B.E., M.A., LL.D., F.R.S.,
M. Inst. C.E.

Mr. C. W. Clarke, of Bombay, referring to Dr. Davies's Paper, pointed out that it was stated that the composite tire profile shown in *Fig. 20* (p. 250 §) was adopted in order to increase the amount of wear necessary to make the tire fit the rails. Although a composite coning of the tire profile might result in better riding of a coach, he did not think the composite tire profile was introduced with that object in view. The chamfering of the outside portion of the wheel tread had been standard practice in America for some time, and was adopted in order to produce more even wear on the nose and wing rails of standard crossings, and consequently to increase the life of crossings. With the ordinary coned tire it was found that considerable wear of the wing rail and battering of the nose in crossings took place, owing to the varying profiles of tires between the form when new and that of a tire worn to condemning profile. With a composite coning of the tire, as wear took place the part with the 1 in 20 slope extended into the 3 in 20 slope. The extension of the 1 in 20 slope, due to wear, tended to distribute the pressure between the tire and rail more evenly on the nose and wing rails of standard crossings, the wing rails of which were ramped to accommodate the 1 in 20 slope of a new tire.

A composite tire profile of 1 in 20, chamfered on the outside to 1 in 8, was adopted as the Indian Railway Standard for both broad- and metre-gauge stock in 1932, in order to increase the life of crossings.

He drew attention to the “Duplex” bogie coach constructed by the Swiss Locomotive and Machine Works. Besides having independently-rotating wheels, those bogies had the wheels canted inwards so as to be

† Journal Inst. C.E., vol. 11 (1938-39), p. 224 (March 1939).

†† *Ibid.*, p. 262.

§ Page numbers so marked refer to the Papers. (Footnotes († and ††) above.)—
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normal to the centre-lines of the rails, which were canted inwards about 1 in 20. The result was that cylindrical tires could be used and the inclination of the wheels and rails gave the desired amount of centering action.

In the introduction to the Paper it was stated that railway vehicles, when travelling at high speed, tended to develop a lateral oscillation. He felt that that was not accurate, as he knew of cases where lateral oscillation was most marked at speeds between 35-40 miles per hour, but was damped considerably at 60 miles per hour.

Also, it was stated that the only cure for bogie-hunting was to withdraw the vehicle from service and re-turn the tires to their original profile. Had any damping or controlling arrangement to prevent bogie-swivelling and the lateral sliding of the wheel tread across the rail-table been attempted?

Referring to Professor Inglis's Paper, Mr. Clarke observed that the curves showing the wheel-paths appeared to be the loci of the points of contact between the tread of the wheel and the rail. The actual paths of an axle moving slowly along a continuous rail would be different from those shown in *Fig. 2* (p. 264 §), as the axle-descent would be increased owing to the elasticity of the wheel-centre and tire. The axle-descent, he thought, was a measure of the resistance to rolling of the wheel (total rolling resistance less journal friction), as it indicated the continuous slope the wheel had to climb as it moved along the rail. In his opinion, one of the greatest practical advantages of using stiffer rail and wheel sections was the reduction in the effort required to overcome rolling resistance, especially at starting, and he thought the effect of a stiffer rail in that respect was as great as that claimed for roller bearings when substituted for plain bearings. At the same time, it was equally necessary to use stiffer wheel-centres, and such examples as the "Boxpok" wheel-centres (cast steel disk-wheels) fitted to the latest heavy American locomotives, and the triangular rim section for spoked wheel-centres, used in the latest British locomotives and the locomotives of the German State Railways, illustrated the trend in development. He noted that the Paper showed that speed had no dynamic effect on a rail-joint in perfect condition. However, he had often found rail-joints with more than a $\frac{3}{8}$ -inch gap between the rails, and with one end of a rail standing $\frac{1}{8}$ inch proud of the adjacent rail. In the case of 5-foot 6-inch gauge wagon stock, the tare weight was less than 12 tons, and the weight of a pair of wheels, axle, and axleboxes was almost 2 tons. In such cases, at a track speed of 45 miles per hour, the wheel-movement could exceed $\frac{5}{8}$ inch, and the vertical acceleration could exceed 30g.

Apart from any question of rail impacts or bad riding, the vertical acceleration of the wheel contributed largely, in his opinion, to the excessive wear of horncheeks; stiffer rails would help to reduce that wear.

Mr. W. J. Doak, of Brisbane, referring to the question of coned versus cylindrical treads, thought it relevant to point out that on the Queensland Railways cylindrical treads and level rails had always been the practice. It was interesting to note, therefore, in engineering periodicals that the authorities had found it advisable to reduce the coning of wheels and canting of rails for the running of the Coronation Scot in England.

There was little doubt that coning was originally adopted to facilitate the withdrawal of patterns from moulds; the rails were then canted to correspond, and conservatism prevented any change when cast-iron wheels were abandoned. He had never been convinced of the necessity of having so much clearance between wheel flanges and rail heads, and thought, moreover, that there seemed to be a very good case for making rails with the heads flared out to the same angle as the wheel flanges.

Railway men had seen the sharp top corners of some sections of rails worn off in a very short time by the intense shearing effect of the fillet connecting treads to flanges. Common sense suggested that new rails and new wheels should have approximately the same profile at the surfaces of contact, and if, then, unnecessary clearances could be reduced, lateral oscillations could probably be rendered negligible.

The revolutionary idea of having independently-rotating wheels had more reason behind it than simply improving the passage around curves and minimizing wear. Fixed axles would be much more trustworthy than rotating ones because of the elimination of stress-reversal. It was well known that axles had to be made of comparatively enormous size because of that stress reversal, and that even then fatigue cracks developed.

Those cracks were accentuated by the damage done by press fitting, and elaborate precautions were taken in surface rolling to minimize that trouble.

Mr. Doak was of the opinion that the advent of roller bearings should revolutionize design, and he pointed out that any such reduction of unsprung weight would be of great value.

Dr. Davies, in reply, stated that he had the "Duplex" bogie in mind in referring to "a bogie having independently-rotating wheels" (p. 251 §); he had examined and travelled in a coach fitted with those bogies. The design possessed undoubted advantages, but he believed that the extra cost was considerable.

He had been at some pains to find a satisfactory device for suppressing the lateral motion of the wheels. That illustrated in *Fig. 19* (facing p. 227 §) had been successful on the model, but had not effected much improvement in practice. The failure might possibly have been due to the plays of the journals in the brasses, which could not be entirely eliminated; if so, the device might be more successful with axleboxes having roller bearings.

Professor Inglis observed that he had no reply to make.

Paper No. 5195.

"On the Problem of Stiffened Suspension-Bridges, and its Treatment by Relaxation Methods." †

By RONALD JOHN ATKINSON, B.E., and Professor RICHARD VYNNE SOUTHWELL, M.A., F.R.S.

Correspondence.

Mr. R. F. C. Booth thought it too early to predict the possibility of sensible economies due to a consideration of the "*u*-effect". Even if, however, such economies were to prove negligible, the investigation would still be one of those essential contributions of mathematics which strengthened the foundations upon which the designer's art was based. That point was important because there was a noticeable tendency amongst practising engineers to decry occasionally a refinement of theory, on the ground that its effect might be buried beneath the mass of mechanical uncertainties which existed in any large structure.

In spite, however, of the development of more accurate methods of analysis, it was important to keep in mind the very real virtues of the elastic theory, most elegantly applied to the problem of the stiffened suspension-bridge by Max am Ende and Dr. D. B. Steinman. That theory enabled the structure to be regarded, not as a wanton breaker of Hooke's law, but as an entity whose elements all strictly obeyed that law. The divergence of the structure, as a whole, from the principle of superposition was to be regarded as a measure of the errors introduced by the use of that theory; those errors were then to be corrected by the application of an appropriate factor to the results. In the light of elastic theory, a stiffened suspension-bridge of given proportions and properties lay at a definite point somewhere between a totally stiffened bridge, for which that theory gave a correct result, and a totally unstiffened bridge, where the maximum error occurred. That point might be defined as the "degree of flexibility" of the bridge, to which a numerical value was easily assigned. That was am Ende's symbol "*D*", or Steinman's "*N*." If proper curves or Tables were constructed for permanent use, then values for *H*, bending moments, shears, and deflexions were immediately forthcoming for various loadings in terms of "*D*" or "*N*." Those results might then be corrected by the factors mentioned, which had been obtained by Steinman from a wide experience of the design of large bridges.

† Journal Inst. C.E., vol. 11 (1938-39), p. 289 (March 1939).

Results having a surprising degree of accuracy would be obtained. In that way a large bridge might be brought to an advanced stage of design within a few hours, with the knowledge that a check by the deflexion theory would lead to minor changes only.

If it were shown that the Authors' refinement led to practical economies, it was to be hoped that it would prove possible to link their theory empirically to the elastic theory, just as Dr. Steinman had linked thereto the currently accepted form of the deflexion theory.

Professor C. E. Inglis, Vice-President, observed that the Authors had indicated how a displacement which hitherto had not been included in suspension-bridge calculations could be taken into account, although the cost of doing so was admittedly very great—so great, indeed, that very few would have the ability or the perseverance to adopt the Authors' method.

Since so much of their mathematics had to be taken on trust, he wished that the Authors had seen fit to illustrate the findings of their intricate analysis by numerical examples worked out for particular cases, so that it would have been possible to appreciate at a glance how far results deduced by the more exact theory differed from those obtained by the simpler methods generally employed. Without such evidence there was a feeling that the discrepancy for a normal form of catenary was not likely to be very noticeable, and possibly would be almost entirely masked by uncertainties in the flexibility of the cable-towers and the rigidity of their foundations.

The Authors asked engineers to take their analysis on trust, and from an inside knowledge of their reliability he was quite prepared to do so, but others might not be so trustful, and the average engineer would certainly hesitate to take advantage of the findings of mathematical processes which he could not even faintly comprehend. The practical engineer liked to keep his feet on the ground, and comparatively crude methods of analysis, as long as they did not take him out of his depth, were preferred to mathematical refinements which were beyond ordinary human reasoning.

For the determination of the primary stresses in frameworks, whether they were statically determinate or not, he rather resented the intrusion of differential equations, and for determining stresses and deformations in a suspension-bridge with a stiffened roadway he believed that a perfectly straightforward method of attack could be adopted. As the Authors pointed out, the difficulty of the problem lay in the fact that Hooke's law and the principle of superposition did not apply, but that difficulty could always be overcome by employing a step-by-step process, the load whose effect was considered being applied in successive increments. For each increment (and probably not more than three would be necessary), the added stress could be computed on the assumption that Hooke's law applied, and from the added stresses thus determined, the consequential change in

the form of the structure could be deduced. The technique required involved nothing more than the ordinary standard analytical or graphical methods for determining total stresses and displacements, whilst tower-movements as well as elongations due to stress and temperature could all be taken into account. There was no need to consider which displacements should be included and which should be left out, for all could be included with equal facility, and, although the actual labour involved would certainly be considerable, it was felt that that would be more than offset by the directness and honesty of that perfectly straightforward method of attack.

Professor Southwell, in reply, expressed his regret that it had not been possible to consult with Mr. Atkinson, who had been mobilised with the R.A.F. That meant that he had to be held solely responsible for the remarks which followed.

He owed to Professor Inglis almost the whole of his early training in structural theory, and on that account it was with great regret that he found himself now in disagreement with almost all of what Professor Inglis had written. But it would serve no purpose to conceal the fact, and he could only state the position as he saw it, hoping that what was faulty in his statement would in time be proved erroneous, and that what (if anything) was of value would survive. The crux of the problem, as Professor Inglis had agreed, was the circumstance that Hooke's law and the principle of superposition did not apply. What was in question was how best to meet that difficulty.

The Authors' procedure had been (like that of Professor Timoshenko before them) to face the difficulty squarely and to show by a worked-out example that although the governing equation was not linear (so that solutions might not be superposed), it could yet be solved with all necessary accuracy in any case that might be presented. Their relaxation method was an alternative to Professor Timoshenko's method of computation, having, as they thought, some advantages; it could equally well have been applied to Professor Timoshenko's equation (3)—which, like their own equation, was non-linear—were it not for their belief that in it they had detected and corrected a flaw. That fact had necessitated two excursions into mathematics instead of one; for not only had the governing equation to be discussed from the standpoint of computation, but it had itself to be corrected by what seemed a more exact treatment of "*u*-effects". The cost of that might be great, but it had been defrayed entirely by the Authors, since the effect of their work for the computer was (*vide* section 17, p. 304 §) no more than a change of constants.

When Professor Inglis proposed a step-by-step method of solution, presumably he meant that as an alternative to the Authors' relaxation method of numerical computation. If so, however, it would be equally

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 11 (1938-39), p. 289 (March 1939).—SEC. INST. C.E.

an alternative to Professor Timoshenko's method—and to any “methods generally employed”; since it started from equation (3) or from the Authors' amendment of that equation, it would be open to the objection (from Professor Inglis's standpoint) that a differential equation would have “intruded” into a statical problem; and why (it might be asked) should anyone look for yet another method, having already two that yielded results?

If, on the other hand, Professor Inglis's step-by-step process was meant to replace the Authors' discussion of “ u -effects”, Professor Southwell could only record his belief that the difficulties presented by the flexible catenary would not be overcome merely by taking them, as it were, in three gulps. (A cable and stiffening girder did not together constitute a framework, and in his memory of Professor Inglis's lectures those had recourse to differential equations when dealing with either singly: why, then, when cable and girder were associated, should differential equations be resented?) However that might be, he had to contest an assertion made twice by Professor Inglis, that the Authors asked engineers to take their mathematics on trust: that he had found really incomprehensible. What, he would ask, could be done with mathematics except to present it in such detail that every step could be followed, and any error detected, by any reader having both the knowledge and the perseverance? That the Authors had done, and accordingly there was no question of their asking the “average engineer” to take results on trust: he might elect to do so (just as he might elect to take Euclid's theorems on trust), but that was his own affair.

Assuming that an engineer could be found with the patience to check their work, and assuming that he found it correct, the question, was all that elaborate analysis worth while, would of course still remain. The Authors believed that it was, on the ground that, although it was desirable to have simple methods, and still more desirable to know that they yielded trustworthy results, before that could be known they had to be tested, and test presumed a theory that was accurate, however complicated it might be. Professor Southwell was delighted to find that point of view expressed (and far better) in the opening remarks of Mr. Booth.

He entirely agreed with Mr. Booth that the need now was of comparisons between different methods; but that meant, of course, much work. It would not now, he feared, be done by Mr. Atkinson, and he could see at present no prospect of being able to do it himself; but he looked for such work to America, where interest in the problem was widespread. With Mr. Booth, he hoped that it would prove possible to link the new theory empirically with the elastic theory; and if that hope were also shared by American engineers, he had little doubt that the comparisons would be forthcoming.

Students' Paper No. 946.

"A Survey of the Present Position in Road
Transition-Curve Theory." †

By DENNIS FRANK ORCHARD, B.Sc. (Eng.), Stud. Inst. C.E.

Correspondence.

Dr. Alexander Thom thought that considerable time could be saved by the use of his Standard Tables *, which had apparently been overlooked by the Author. Those Tables were based on the clothoid, so that identical results should be obtained by their use as by the use of equations (5) (p. 334 §) and (10) (p. 337 §) and other expansions given by the Author. The latter were, in Dr. Thom's opinion, quite unsuitable for routine field work. It was that consideration which engendered the idea of preparing standard Tables in the first place. Facilities for finding X and Y were included, thereby obviating the use of the expansions given on p. 339 §. For the straightforward work, however (such as the examples given in the Appendixes to the Paper), it was not necessary to obtain X and Y at all, since Dr. Thom had given direct expansions for the shift and subtangent, and had prepared a Table for application to all ordinary cases. To illustrate the saving effected by those Tables Dr. Thom applied them to the example in Appendix I (p. 351 §).

The rate of change of acceleration was 1.15 feet per second per second per second. In the notation of the Standard Tables

$$\text{acceleration} = \frac{V^2}{R} = \frac{V^2 S}{k^2} = \frac{V^3 t}{k^2},$$

and rate of change of acceleration

$$= \frac{V^3}{k^2}.$$

† Journal Inst. C.E., vol. 11 (1938-39), p. 327 (March 1939).

* "Standard Tables and Formulæ for Setting out Road Spirals." Pitman, London, 1935.

§ Page numbers so marked refer to the Paper (Footnote (†) above).—SEC. INST. C.E.

Therefore
$$k = \sqrt{\frac{V^3}{1.15}} = 419.023.$$

The minimum value of R was
$$\frac{V^2}{0.3g} = 356.292 \text{ feet,}$$

and, since
$$LR = k^2,$$

then
$$L = 492.804 \text{ feet.}$$

Using small letters for the basic curve,

$$l = \frac{L}{k} = 1.17607,$$

and the angle turned through in the transition was

$$\frac{l^2}{2} = 0.691567 \text{ radians} = 39 \text{ degrees } 37 \text{ minutes } 25 \text{ seconds.}$$

Up to that stage the calculation followed that of the Author, the difference being merely in the presentation. For the deflexion angle (Table A, p. 353 §) corresponding to $S = 300$ feet, using again the notation of the Standard Tables,

$$s = \frac{300}{k} = 0.71595.$$

With only a simple interpolation, Table I (p. 22 §§) gave directly 4 degrees 53 minutes 32 seconds, which agreed to within 1 second with the Author's value. When the instrument was brought forward to station 300, the back angle was $\frac{s^2}{2} - \phi$, and the forward angle was approximately $\frac{1}{2}sc + \phi_c$ as given by the Author. It should be noted, however, that the forward angle was really $\frac{1}{2}sc + \phi_c - \delta$, where δ was a correction which might, in some circumstances, be quite appreciable. The correction arose from the slight inaccuracy of the osculating-circle method. That was fully discussed in the Standard Tables. In the example the deflexion angle between stations 300 and 450 was

$$\begin{aligned} \frac{1}{2}sc + \phi_c - \delta &= (7^\circ 20' 32'') + (1^\circ 13' 25'') - 4'' \\ &= 8^\circ 33' 57''. \end{aligned}$$

§ *Ibid.*

§§ Page numbers so marked refer to Dr. Thom's book (Footnote (*), p. 457.)—
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The shift and subtangent were easily obtained :

$$\text{Shift} = N = \frac{L^2}{24R} - k \Delta n ;$$

$$\text{Subtangent} = (R + N) \tan \frac{1}{2}\alpha + \frac{L}{2} - k \Delta m.$$

Table III (p. 27 §§) then gave

$$\Delta n = 0.00117 \text{ and } \Delta m = 0.00936,$$

the application of which gave

$$\text{shift} = 27.91 \text{ feet and subtangent} = 731.19 \text{ feet,}$$

agreeing with the Author's results. It could be seen that the Standard Tables gave the same results as the Author obtained, in a fraction of the time. Furthermore, they enabled more complicated cases, such as the transition between compound curves, to be dealt with.

Dr. Thom pointed out that the examples were not worked so as to obtain the running chainage, and he thought that that did not receive sufficient consideration in Great Britain.

Mr. H. A. Warren referred to the Author's deprecation (p. 328 §) of certain empirical methods used in the design of railway transition-curves.

He pointed out that the shift, which might be evaluated from $\frac{1}{24C^2} \left(\frac{V^2}{R} \right)^{\frac{3}{4}}$, allowing a maximum value of 8 feet per second per second for $\frac{V^2}{R}$,

and using values of C recently experimentally obtained*, became negligible. On that basis alone the transition could be dispensed with.

The fact remained, however, that the superelevation had to be gained gradually, and so the length of the transition became, once more, a function of the rate of change of superelevation. The empirical rules regarding rate of gain of superelevation mentioned by Mr. Orchard on p. 328 § were not therefore to be entirely despised. Where, however, the track was for a highway and not a railway there were many other variables to be considered. Upon the entry of a road vehicle into a transition-curve there would be forces arising from vertical accelerations due to the application of superelevation, and also due to rotation about a longitudinal axis consequent upon the increasing angle of banking. In evaluating those,

§§ Footnote (§§), on p. 458.

§ *Ibid.*

† H. A. Warren, "Highway Transition Curves." Journal Inst. M. & Cy. E., vol. lxx (1938-39), p. 311 (August 16, 1938).

* H. A. Warren and E. R. Hazeldine, "Highway Transition Curves." Journal Inst. M. & Cy. E., vol. lxx (1938-39), p. 1021 (March 28, 1939).

the width of the road, the method of tilting the surface about one edge or the centre line, the wheel-base, and the track-width of the vehicle would be involved. The accelerations would be present only in the small interval of time between the passage of the front and rear wheels across the line at which the rate of gain of superelevation commenced, and the provision, or otherwise, of a vertical curve there would affect the values. Those were the considerations which set a limit upon the value of C to be adopted, except that at low speeds and with steering gear ratios of about 6 to 1, the rate of turning the steering column might be the limiting factor. Very rarely indeed, if ever, would the rate of gain of radial acceleration itself, manifested as an increasing centrifugal force, produce either discomfort or danger so long as the ultimate value of $\frac{V^2}{R}$ was restricted to about 8 feet per second per second.

On p. 329 § the Author stated that Professor Royal-Dawson had provided a graduated empirical formula providing for an increase in the rate of gain of centripetal acceleration for speeds below 30 miles per hour. Recent experimental work had shown the reverse to be the case, borne out by theory on the assumption of a natural rate of turning the steering column being maintained constant at all velocities †. It would be of interest if the Author could state his reasons for having expressed the opinion (p. 330 §) "that Professor Royal-Dawson's values are at present the best compromise." It was unfortunate that such misnomers as "increased manoeuvrability" and "standard rate of turning" should be reproduced in the Paper as affecting, or meaning C , the rate of gain of radial acceleration; the former had no scientific meaning, and the latter was a function only of radius of curvature and velocity, and had no necessary connexion with transition-curves at all. Such expressions led only to confusion.

On p. 341 § the Author used the "unit-chord" advocated by Professor Royal-Dawson, and defined as "the length of polar ray whose polar deflexion is 16 minutes." That peculiar and arbitrary unit had its main justification in a slight simplification in the calculations for deflexion angles not greater than 4 degrees; that was to say, only within the limits where the curve would be as accurately and more simply defined by the cubic parabola equation. The natural parameter for use with the lemniscate was the maximum polar ray, at 45 degrees, and the introduction of unnecessary units such as "unit-chords" and "speed-chords" only made a perfectly straightforward subject appear unnecessarily confused.

Mr. Robert Young thought that the Paper was a useful survey of what had been done regarding the design of road curves, and would be useful to engineers who wished to acquaint themselves with the salient

§ *Ibid.*

† Warren and Hazeldine, p. 1025. (Footnote (*), p 459, *ante*.)

features of the subject with a minimum of investigation. The calculations set forth in the Appendixes to the Paper would hardly be undertaken by engineers wishing to set out transitions in the field.

It would never be possible to cater for road traffic with the same degree of certainty as for rail traffic. Some attempt, however, should be made to anticipate the conditions for which the curves were to be designed, with a view to approaching, as nearly as possible, that degree of certainty. He advocated the addition to signposts of figures showing the maximum speeds at which the bends should be negotiated. Those speeds would be compatible with the speeds for which the curves were designed and with the nature of the road surface. A distinction should be made between the maximum safe speed for a dry road and that for a wet road, both speeds being displayed on signs at the roadside. He considered that the white line, generally used, should be supplemented by a 6-inch by 4-inch curb to prevent vehicles, whilst negotiating the bend, from encroaching on the wrong traffic-lane. That precaution was especially desirable at sharp bends.

Mr. Young thought that greater superelevations could be employed on sharp curves on main roads in country districts. It would be seen from the expression governing superelevation, that the centrifugal force was directly proportional to the square of the speed and inversely proportional to the radius of the curve. Owing to greater visibility on the outside of the curved carriageway, the speed of vehicles might be appreciably in excess of that of vehicles on the inside half. The superelevation might, therefore, be increased on the outside half of the carriageway. In the case of a curved carriageway with parallel kerbs the superelevational change could be defined by the 6-inch by 4-inch kerb referred to, or, if the outside and inside radii were the same, the two superelevations could be separated by the central reserve given by the consequent widening for carriageways of similar width. Horse-drawn traffic should be discouraged and cyclists segregated completely. Mr. Young pointed out that on the *Autobahnen* in Germany, all traffic other than motor vehicles was prohibited.

In view of the fact that careful calculations had been evolved with a view to defining the road surface in line and level, and methods used to put the results of those calculations into practice, it was Mr. Young's opinion that, in most cases, the conditions, as designed, were only temporary, and other conditions (often dangerous) resulted from settlement of the sub-grade. In order that the surface, as designed, could be maintained as permanent as possible, as much attention should, in the future, be given to the soil mechanics of road formations, as in Germany and the United States.

The Author, in reply, indicated that he was aware of the existence of Dr. Thom's Standard Tables, and agreed with Dr. Thom that the use of Tables did save time and that they were essential for routine field

work. He did not consider, however, that those Tables were sufficiently complete for that purpose. He pointed out that the title of the Paper indicated that its sole purpose was to present the pure theory of transition-curves, in the hope that the gaps left by existing Tables would thereby be filled.

The extreme accuracy which might in certain cases be required, such as in the setting out of long tunnels, could hardly be obtained from existing Tables, but was readily available by the method set forth in Appendix II of the Paper. Dr. Thom's method of calculating the first example was very interesting but he had omitted the more laborious parts of the process which recourse to his Tables did not entirely and satisfactorily remove. Dr. Thom's Table I §§ gave the x and y co-ordinates and the polar deflexion angles for certain intervals of length round the spiral, all the linear dimensions being given in terms of a unit and thus requiring transposition to practical dimensions. Unless, therefore, the engineer made, when using those Tables, the same assumption that the Author made in the working of the first example in Appendix I (but not in the working of the example in Appendix II), namely that the chord length and arc length were equal, the inconvenient offset method of setting out was the only one available. Dr. Thom appeared to object to such an assumption on the ground that it did not retain the running chainage. Furthermore, Dr. Thom's book did not give any tabular information of the deviation for the different polar deflexion angles, of the length of the transition for a particular design speed, or of the minimum radius and centrifugal ratio, where those did not reach their limiting values. In those circumstances, the Author could not concede Dr. Thom's claim that his Tables gave the same information as the Author's calculations.

The Author had deprecated, as unnecessary, existing empirical methods for the setting out of transition-curves and, in particular, for assessing their length or the necessary distance for the introduction of superelevation. He would point out that Mr. Warren's value of 8 feet per second per second for $\frac{V^2}{R}$, although suitable for road curves, was quite unsuitable for railway work, and although a reduction of that to the more usual value of 4 feet per second per second would result in a still smaller shift, there was no reason whatever for assuming that the values of C as determined in Mr. Warren's experiments were applicable to railway practice, even they were considered seriously for road work. For that and other reasons it did not appear that his contention that transition-curves could be dispensed with was basically correct. Furthermore, the rate of change of superelevation was a variable which could most conveniently be kept subservient to the theoretical length of transition, and not the reverse as Mr. Warren contended. The entire basis of the modern theory was

§§ Footnote (§§), p. 458.

the necessity for some method of calibrating the sharpness of a transition curve. That was done by using the value of C in conjunction with the design speed of the curve, enabling the scale of the transition-curve to be determined, whether that was measured by the American Degree system or by Professor Royal-Dawson's Unit Chord system. If that method were adopted, the distance over which the superelevation was to be applied could be calculated quite simply from the theoretical principles and without recourse to empirical rules.

For instance, a rate of gain of centripetal acceleration of C feet per second per second per second was equivalent to a rate of gain of centrifugal ratio $\left(\frac{W}{P} = B\right)$ of $\frac{C}{g}$ per second.

If W denoted the width of the road,
 v " the speed of the vehicle in feet per second,
 and k " the proportion of the centrifugal ratio which was at any instant to be taken up by superelevation (Professor Royal-Dawson suggested a value of 0.4),
 then the rate of gain of crossfall or superelevation was, at any time, 1 in $\frac{g}{kC}$ per second. The final crossfall, however, was to be 1 in $\frac{1}{kB}$, and therefore the time (t seconds) taken to reach that superelevation was

$$\frac{g}{kC} \div \frac{1}{kB} = \frac{gB}{C},$$

and the distance over which the superelevation should be applied was

$$\frac{vgB}{C} \text{ feet.}$$

Furthermore, it could easily be shown that if the superelevation were applied about the centre line of the road, then the gradient of the outside and inside edges of the road would be 1 in $\frac{2vg}{WCk}$, and that might be added, with due regard to sign, to any existing gradient of the road.

The precise effect of the large number of variables mentioned by Mr. Warren as limiting the value of C did not admit of analytical treatment, and recourse had to be made to experiment to determine the value of C which might be adopted. With that in view, and in order to obtain information for a research on skidding, the Author had conducted, before the publication of Mr. Warren's experiments but after the writing of the Paper, a series of tests employing exactly the same method of tracing out the curves as did Mr. Warren. The experiments were conducted mainly with a small car, but a pedal-cycle was also used. It was thought that a pedal-cycle would act as a valuable standard since it was the lightest and the most easily manoeuvred of all road vehicles, and possessed the

capability of banking and thereby obtaining the effect of perfect super-elevation.

The preliminary conclusions arising from those experiments were, to a certain extent, the same as Mr. Warren's, but in the absence of similar experiments conducted with a heavy road vehicle such as a double-decked bus, the Author was of the opinion that it was necessary to exercise rather more restraint in drawing conclusions than Mr. Warren appeared to have done. It was agreed that the possible rate of turning of the steering column might be a limiting factor in fixing the value of C . Against that, however, it was found that the value of C obtained with a pedal-cycle was not generally greater than that obtained with the car in spite of the speed with which the steering wheel of the pedal-cycle, for mechanical reasons only, could be turned. The value of C obtained in comfort did show a general tendency to increase as the vehicle speed increased, but the increase was not merely proportional to the square of the speed, as would be the case if it were assumed that the rate of turning of the steering column remained the same in all cases. The Author agreed, however, unreservedly with Mr. Warren that the value of the centrifugal ratio affected the comfort of turning to a far greater extent than the value of C , but he thought that the figure of 0.25 for the limiting centrifugal ratio could, subject to the possibilities of skidding, be increased materially without causing discomfort. For the value of C the Author had obtained, generally, high values, and could state that a value of 6 feet per second per second per second was quite practicable, although not to be recommended for general use. He had dismissed as unreliable any values much in excess of that figure, but he thought that Mr. Warren had not been quite so reserved. Contrary to Mr. Warren's expressed opinion, he did not find the method of determining experimentally the value of C adopted by both, to be reliable at speeds in excess of 25 or, at the most, 30 miles per hour.

The reason for stating (p. 330 §) "that Professor Royal-Dawson's values are at present the best compromise" was that at the time that that observation was made no experimental data existed beyond Mr. Shortt's work, and in the absence of such it was better to err on the safe side.

The "manœuvrability" of a vehicle depended, amongst other factors, on its weight and its tendency to body-sway, and therefore it directly affected the possible "rate of turning"; the Author could not, therefore, understand Mr. Warren's contention that they were entirely independent of C , the rate of gain of radial acceleration, nor would he accept the statement that the rate of gain of radial acceleration had no connexion at all with transition-curves.

There was no objection to the use of the maximum polar ray of the lemniscate as a unit of measurement, except that it was rather large;

it was just as arbitrary as Professor Royal-Dawson's unit, which the Author had adopted as he saw neither wisdom in, nor necessity for, adding a further unit to those already in use.

The Author agreed with Mr. Young's suggestion that the safe speed of a curve should be indicated on a signpost at the beginning of the curve; there was, however, a limit to the number of road directions which a driver could be expected to read—a limit which would prohibit the addition of a separate safe speed for wet conditions. The effect of rain in lowering the coefficient of friction was a very variable factor, and the ideal to be attained was a road surface which was equally safe when wet as when dry.

Paper No. 5187.

"Wind-Pressure Experiments at the Severn Bridge." †

By ALFRED BAILEY, M.Sc., Assoc. M. Inst. C.E. and NOEL DAVID
GEORGE VINCENT, B.Sc.

Correspondence.

Mr. G. S. Gough observed that the experiments described provided some important new information regarding the width of action of the simultaneous rise of pressure in a gust, but that they did not throw very much light on the distribution of pressure; and whilst there was, perhaps, no final reason to disagree with the Authors' conclusions, the evidence in support lay not so much in those experiments, or in those of Sir Thomas Stanton, as in the American series described by Messrs. R. M. Sherlock and M. B. Stout*.

In the first place it was important to realize that the fundamental problem was one which might be stated thus:—Given the maximum wind velocity (or pressure) to be expected at any point in the locality over a long period of time, for what mean pressure should allowance be made in the design of a structure of given dimensions of elevation?

Now in the Authors' first series, and in the Tower Bridge experiments, simultaneous measurements were made of the mean pressure over a length and of the pressure at a point within the length, and the interpretation was to some extent based on the instantaneous ratio of mean pressure to point pressure, which might appear to bear directly on the problem stated above, but only did so when the point pressure was instantaneously the greatest within the area of the measurements. That could only be fortuitous. Nor could any repetition or prolongation of the experiments discount the element of chance, as on the above argument the smaller ratios were more likely to represent the required ratio of mean pressure to maximum point pressure; the maximum value of the ratio was, however, wanted, and it was impossible to reconcile the opposing considerations. In fact, such diagrams as *Figs. 14 and 15* (p. 375 §) were not very enlightening. In any event the required ratio could not exceed unity.

Diagrams such as *Figs. 12 and 13* (pp. 372–373 §) could, however,

† Journal Inst. C.E., vol. 11 (1938–39), p. 363 (March 1939).

* "Picturing the Structure of the Wind." *Civil Engineering*, vol. 2 (1932), p. 358.

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 11 (1938–39), p. 363 (March 1939)).—SEC. INST. C.E.

be interpreted in a way which seemed to be open to less objection. In *Fig. 13* the greatest group pressure of section A was about 8.2 lb. per square foot and the greatest from the single instrument about 10.6 lb. per square foot, not at the same instant. Those pressures were in the ratio of 0.77 to 1. Now the former pressure was a true maximum for the period of the record, apart from any error due to the mean pressure at five points not being an accurate measure of the mean over the area; but the point pressure of 10.6 lb. per square foot had no such claim to be a true maximum, as it was quite unlikely that the point of maximum pressure coincided with the position of the single instrument. It was permissible, therefore, to state that for that period of time the ratio of the greatest mean pressure to the greatest point pressure did not exceed, and was probably less than, 0.77. For section B the corresponding ratio was 0.88; and those of the published Tower Bridge records ‡, to which the method could be applied, gave ratios ranging from 0.65 to 1.02.

Those figures all suggested strongly that the mean pressure over the 1,340 feet of the Severn bridge, or over the 170 feet of the Tower bridge, was considerably less than that which would be deduced from the reading of a single anemometer, the ratio being, perhaps, of the order of from 0.75 to 0.80. Unfortunately, the diagrams reproduced in "Picturing the Structure of the Wind" * did not support that view, and placed the reduction at a smaller percentage.

A fairly elaborate method had been used to determine the mean pressures corresponding with those diagrams. Over ranges of lengths and of heights the ratio of mean pressure to maximum point pressure was found to be:—

In horizontal distribution, on lengths of	100 feet . .	1.0
	200 feet . .	0.95
	400 feet . .	0.90
In vertical distribution, on heights of	50 feet . .	1.0
	100 feet . .	0.95
	150 feet . .	0.90

Those ratios were mainly associated with flat-topped and double peaks of pressure occurring in gusts of less than the maximum intensity, and the figures could all be reduced by 0.05 if it could be assumed that the highest peaks were never of that character. Unfortunately, there was insufficient evidence for such an assumption.

The conclusion, therefore, which could finally be drawn from all those experiments, was very much that which Sir Thomas Stanton had deduced; namely, that whilst the mean pressure was less than the point pressure,

‡ "Report on the Measurement of the Pressure of the Wind on Structures," Minutes of Proceedings Inst. C.E., vol. 219 (1924-25, Part 1), p. 125.

* Footnote (*), p. 466.

the amount of reduction which could be relied upon was insufficient to be of practical importance.

Mr. Gough wished to acknowledge that the above ideas were in the first place not so much his own as the result of discussion of that subject by the Joint Committee of The Institution and of the Institution of Structural Engineers on Simply Supported Steel Bridges, and he wished to thank the Committee for permission to publish the results of calculations first submitted to it.

The Authors, in reply, were pleased to note that Mr. Gough agreed with the general deduction.

The fundamental problem, which he had stated clearly, had, of course, been appreciated, and in drawing up *Figs. 14 and 15* (p. 375 §) it was realized that only the highest peak pressure at the single point was of interest; it was felt, however, that plotting all the peak pressures, which of course, included the highest, might give some indication of the trend of the ratio with variation of peak velocity; allowing for the wide scatter, there was some indication that the ratio tended to fall.

§ *Ibid.*

CORRESPONDENCE

ON PAPERS PUBLISHED IN

APRIL 1939 JOURNAL.

Paper No. 5197.

"The Storstrøm Bridge." †

By GUY ANSON MAUNSELL, B.Sc. (Eng.), M. Inst. C.E., and JOHN FREEMAN PAIN, M.C., B.Sc. (Eng.), Assoc. M. Inst. C.E.

Correspondence.

Mr. K. G. Mitchell, of Simla, observed that in the Appendix (p. 428 §) the Authors gave the formula $\frac{1}{R} = \frac{5.5p^2L}{EP}$ for the design of roller bearings, but they did not make it clear what value was assigned to p . British Standard Specifications (No. 153) gave the formula $P = 0.28 DL$, where P denoted the total load on the roller in tons, L the length of the roller in inches, and D the diameter of the roller in inches. Taking that formula and substituting $\frac{1}{4}DL$ for P in the Authors' formula, the value of p became 37.6 tons per square inch, without allowance for tolerance in a battery of rollers.

A formula, which was a different form of that quoted by the Authors, was understood to be in common use on the Continent. It was $D = 0.36 \frac{AE}{bc^2}$, where A denoted the total load, b the total length of all the rollers in inches, and c the permissible compressive stress. He had seen it stated that in that formula certain Continental regulations (including the Danish) allowed a value of 1.2×10^5 lb., or 53.5 tons, for c . There again, no allowance was made for tolerance. That appeared to be altogether excessive. Would the Authors state what value they assigned to p in their formula? It appeared to be quite independent of what preceded it.

Dr. H. J. Nichols, of Bombay, proposed to confine his remarks to the stiffened-arch navigation-spans, the design of which, in his opinion, possessed particularly attractive features. It was stated on p. 407 §

† Journal Inst. C.E., vol. 11 (1938-39), p. 391 (April 1939).

§ Page numbers so marked refer to the Paper. (Footnote (†) above.)—SEC. INST. C.E.

that the weight of steel in the three spans taken together was 3,460 tons, as compared with 3,060 tons for a normal through-type cantilever-design. In view of the absence generally of complicated shop-work in the arch-design, and of simplified erection and maintenance, could the Authors give comparative total costs, including the capitalized value of painting, etc., to represent the comparative outlays involved in the two cases? Could they also give an estimate of the secondary stresses induced at the top and bottom of the shortest (end) vertical hangers under the worst conditions of partial loading? There was nothing to be gained by designing hanger connexions rigid in the plane of the truss, and the experience recorded in respect of the cracking at the ends of some of the longer hangers would suggest another reason for avoiding too great rigidity at those points.

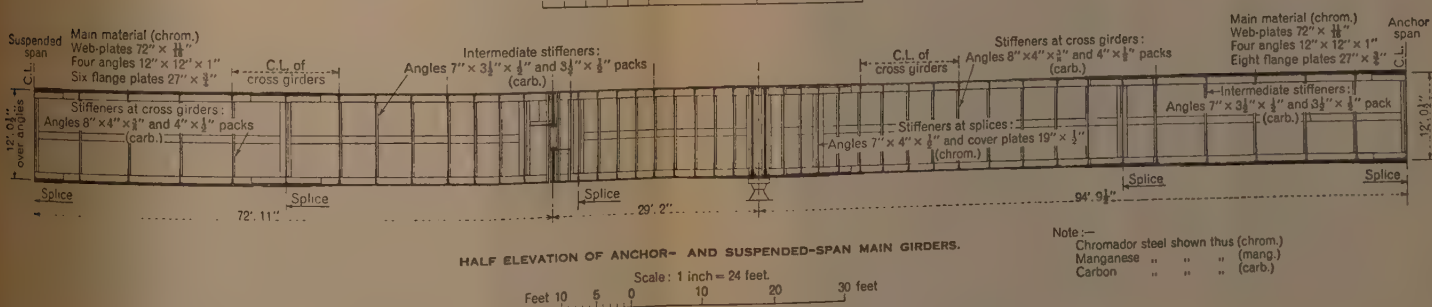
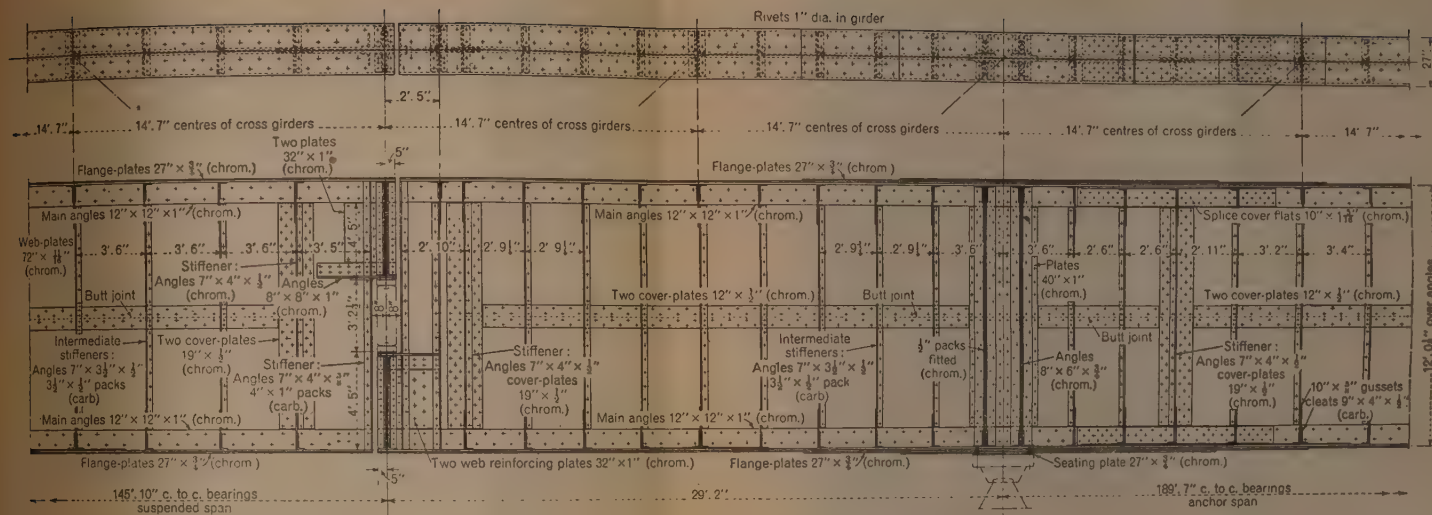
As railway spans, a consideration of controlling importance was the amount of deflexion upward or downward which occurred in the stiffening girders as a capacity load advanced from one end. Could the Authors give any experimental figures in that connexion, together with the calculated deflexions? An interesting feature of that type of design was that a point of contraflexure occurred in the stiffening girder ahead of an advancing load, and moved forward ahead of the load. Synchronous vibrations under railway loadings could not therefore build up. Although that applied to vertical oscillations, it was clear that appreciable lateral oscillations of the top chords could only occur by the twisting of the span as a whole, which in turn involved vertical oscillations of the two stiffening girders. It was to be expected, therefore, that lateral oscillations of the top chords or arch-ribs would be also controlled, and that had been confirmed by the remarks of the Authors on p. 447 §.

With regard to the expansion bearings, mounted on pairs of segmental rollers 28 inches in diameter, it would be of interest to know whether or not that type of bearing was actually cheaper in place than the more usual bearing on a nest of rollers. There appeared to be a considerably greater weight of metal involved, whilst to offset that there was a reduction in the number of rollers to be accurately ground. Although by using pairs of rollers larger tolerances in diameter could be accepted, it was perhaps more necessary that the diameter of each individual roller should be constant within fine limits in order to avoid distortion of the castings.

Lastly, he wished to call attention to the formula for the factor y as applied to battened columns, given on p. 428 §. There appeared to be two misprints which required correction to make the expression homogeneous.

The Authors, in further reply to the Discussion and in reply to the Correspondence, observed that, with regard to the relative areas requiring to be painted on a plate- and lattice-girder design, it had been possible

Figs. 35.



Note:—
Chromador steel shown thus (chrom.)
Manganese " " (mang.)
Carbon " " (carb.)

DESIGN OF APPROACH-SPAN GIRDERS OF STORSTRØM BRIDGE.



to make some further calculations, in the light of which the statement on p. 406 § required some amplification. The ratio of the exposed area of a lattice-girder to that of a plate-girder of similar span was found to be 1·5 to 1 for the main girders only, but, when the whole of the deck and bracing of the Storstrøm bridge (including the approaches) was considered, the ratio was found to be 1·3 to 1.

The elevation of one of the approach girders was given in *Figs. 35*. No special provision had been made to bond the concrete of the road and railway decks to the stringers, but there was no doubt that bonding actually took place. No advantage had been taken of the fact in the design of the stringers.

Detailed total comparative costs for the stiffened arch and lattice-girder design for the navigation-spans, and for multiple and two-roller bearings were, unfortunately, not available.

Referring to the rigidity of the hanger connexions in the plane of the truss, the Authors thought that the use of pin-joints at such points, whilst perfectly feasible, might have introduced an additional source of wear, due to the working of the hangers on the pins; it was difficult to design such a joint so that all parts were accessible for painting. The friction on pins, except those very small in relation to the width of the member connected, was quite sufficient to produce fixity at the joint under the range of secondary stresses normally encountered.

Referring to Mr. Mitchell's observations, the Authors pointed out that the Table on p. 428 § had been printed wrongly and should read:—

Bearings with:	Combination (1), <i>p</i> : tons per square inch.	Combination (2), <i>p</i> : tons per square inch.
One or two rollers	51·0	57·0
Three or more rollers	44·5	51·0
Point-contact	70·0	82·5

The load allowed by the Danish formula on bearings consisting of two rollers was approximately 1·8 times that given by the B.S.S. formula, and 1·4 times that for bearings with three or more rollers. The Continental formula quoted by Mr. Mitchell appeared to be of the same form as that specified by the Danish Authorities, the difference being only in the constant. With the permissible stress stated, the formula gave results about twice as great as those allowed by the B.S.S. for bearings with two rollers. The Authors were not aware of the experimental data upon which the B.S.S. formula was based, and without some evidence of the harmful effect of the high pressures permitted by the Continental formula it would appear unwise to condemn it.

The formula for y (p. 428 §), the factor by which the unsupported length l of battened columns should be multiplied, should read :—

$$y = 1.1 \sqrt{1 + 0.5 \times \frac{I}{I_f} \times \left(\frac{c}{\bar{l}}\right)^2 + T \times \frac{F_f \times C \times h}{5 \times E \times I_t}},$$

whilst in the Table of notation, following that formula, l_f and l_t should read I_f and I_t respectively.

Paper No. 5189.

“Considerations on Flow in Large Pipes, Conduits, Tunnels, Bends, and Siphons.”†

By JAMES WILLIAMSON, M. Inst. C.E.

Correspondence.

Mr. Herbert Addison, of Giza, Egypt, considered that the various Papers on bends and siphons reviewed in the Author's Paper together formed a very interesting study in the dissolution of the belief that the energy loss in a bend depended only on the shape of the bend and not on the surface roughness. The Author had therefore done a valuable service in pointing out again that the flow through a bend was as dependent upon the roughness of the walls as was the flow through a straight pipe.

Mr. Addison's own contribution to the experimental information available, entitled “Experiments on the Flow of Water in Pipe-Bends” * fully supported that conclusion. When comparing smooth brass bends with rough cast-iron bends of identical shape, he had found that, no matter how the component bends of the complete systems were built up, the total energy-loss in the smooth system was always less than the corresponding loss in the rough system; and the ratio between the losses was greater even than the ratio between the losses in comparable straight lengths of pipe. The effect of roughness in increasing the loss through pipe-bends had more recently been discussed in a Paper by Mr. K. Hilding Beijer which described work carried out at the National Hydraulic Laboratory, Washington ‡.

Mr. Addison regretted that reference to the position of a bend relative

§ *Ibid.*

† Journal Inst. C.E., vol. 11 (1938–39), p. 451 (April 1939).

* Paper No. 5084. Abstract published in Journal Inst. C.E., vol. 6 (1936–37) p. 561 (October 1937). [The MS. and illustrations may be seen in the Institution Library.—SEC. INST. C.E.]

‡ “Pressure Losses for Fluid Flow in 90-deg. Pipe Bends.” Research Paper RP111 O.

to adjacent bends or to the inlet and outlet of the conduit had been omitted from the Author's Paper. That point had been raised in the discussion on Mr. Powys Davies's Paper on "The Laws of Siphon Flow" || and the experiments described in Mr. Addison's Paper †† had made more doubtful than ever the feasibility of assessing the energy-loss in a bend independently of the flow-conditions upstream and downstream.

The point raised by Mr. E. R. Howland, and commented on by the Author in his reply to the Discussion, was a fundamental one: in normal flow in a straight pipe, was the total energy uniform across the section or was the pressure uniform? If the ordinary assumption that the pressure was uniform were correct, then the third column of Table I (p. 460 §) was misleading. Would it not be possible to settle the matter by making a traverse with a pitot tube of the combined type, in which the static pressure was measured at a ring of holes in a cylindrical sleeve surrounding the impact-orifice? The same instrument, or possibly a recent development of it known as the "Pitot Sphere", might also yield valuable information about the pressure- and energy-distribution inside curved passages; for as far as could be seen, the multitudinous pressure measurements recorded in the Papers presented to The Institution during the past dozen years related only to conditions at the walls of the passages.

Mr. Thomas Blench expressed interest in the Author's preference for a basic exponential flow-formula, instead of for the von Kármán logarithmic type of formula. The Manning formula, written to suit the Author's argument, was

$$V = (\text{absolute constant}) \cdot (R/k)^{\frac{1}{3}} \sqrt{gRS}, \quad \dots \dots (7)$$

which, physically, was as full of meaning as the von Kármán formula, for it was dimensionally correct, and introduced the relative roughness. Mr. Blench, in his Paper entitled "A New Theory of Turbulent Flow in Liquids of Small Viscosity" *, recommended the formula:

$$V = (\text{absolute constant}) \cdot (R/k)^{\frac{1}{3}} \sqrt{gRS} \quad \dots \dots (8)$$

He had already shown ‡ that, for smooth boundary conditions, that formula was the same as that of Blasius, which was usually written:

$$V = (\text{constant}) \cdot R^{\frac{5}{8}} S^{\frac{3}{8}} \quad \dots \dots (9)$$

|| Minutes of Proceedings Inst. C.E., vol. 235 (1932-33, Part 1), p. 352.

†† Footnote (*), p. 472.

§ Page numbers so marked refer to the Paper. (Journal Inst. C.E., vol. 11 (1938-39), p. 451 (April 1939)).—SEC. INST. C.E.

* Paper No. 5185. *Abstract published in* Journal Inst. C.E., vol. 11 (1938-39), p. 611 (April 1939). [The MS. and illustrations may be seen in the Institution Library.—SEC. INST. C.E.]

‡ *Correspondence on Paper on* "Turbulent Flow in Pipes, with particular reference to the Transition Region between the Smooth and Rough Pipe Laws", by Dr. C. F. Colebrook; p. 393, *ante*.

Considering von Kármán's and Nikuradse's work on turbulent-velocity distribution, it would be expected that, since there was only one kind of turbulence, there should be a general flow-formula for all boundary-conditions, and that the special formulas for different boundary-conditions should be derivable from it by altering the term expressing the nature of the boundary. Mr. Gerald Lacey's formula, based on data which overcame the difficulty of change of material with size of channel, also led to formula (8) above, but with his silt-factor f replacing k ||.

Practically, it was not usually important whether $\frac{3}{4}$ or $\frac{2}{3}$ were used as an index of R . If the performance of a large tunnel had to be prophesied from small-pipe experiments (and if it were certain that the absolute roughness would be the same in both cases), then it would be preferable to use the dynamically-correct $\frac{3}{4}$. No useful range of R in any one channel was, however, sufficient to justify $\frac{3}{4}$ in preference to $\frac{2}{3}$ or some similar value.

Tables IX and X (pp. 489-490 §) were of considerable practical value. If they were adjusted for the index $\frac{3}{4}$, it would be necessary to know the value of R at which n was determined. Mr. Blench had started the compilation of C in the formula $V = CR^{\frac{3}{4}}S^{\frac{1}{2}}$, as that formula was now being recommended for river work in India, and he was using it for designing concrete canals; C would vary inversely as the one-fourth power of k . Table XII gave comparative values.

TABLE XII.—VALUES OF C IN $V = CR^{\frac{3}{4}}S^{\frac{1}{2}}$.

(Using data of Mr. A. A. Barnes's "Hydraulic Flow Reviewed" (London, 1916).)

Material.	C	Barnes's Table.
Rock, faced with masonry in cement	89	XIV
Bikaner canal: Kankar lime concrete, rather undulating	100	Punjab (not Barnes)
Clean hard-brick well-pointed conduits	115	XI
Dressed masonry in cement	123	XIII
Clean neat-cement pipes	158	X

In view of the theoretical merits of both exponential and logarithmic formulas, he had compared them from a purely mathematical standpoint. To make that comparison he had plotted, to the same axes, the curve and points of *Fig. 2* of Dr. C. F. Colebrook's Paper †, the lines fitting Nikuradse's data as given in *Fig. 24* of Professor B. A. Bakhmeteff's book "The

|| "Uniform Flow in Alluvial Rivers and Canals." Minutes of Proceedings Inst. C.E., vol. 237 (1933-34, Part 1), p. 421.

§ *Ibid.*

† "Turbulent Flow in Pipes, with particular reference to the Transition Region between the Smooth and Rough Pipe Laws." Journal Inst. C.E., vol. 11 (1938-39), p. 144 (February 1939).

Mechanics of Turbulent Flow" ††, and Blasius's line (drawn from the consideration that $1/\sqrt{\lambda} = pN^{\frac{1}{4}}$, from equation (9)—assuming that that was correct). That meant that it should be plotted as $1/\sqrt{\lambda}$ against $7 \log (1/\sqrt{\lambda}) - 8 \log p$, where p had to be determined. Accordingly $1/\sqrt{\lambda}$ was plotted against $7 \log (1/\sqrt{\lambda})$, and the whole curve was displaced horizontally to give the best fit by eye to Colebrook's points. From the displacement, it was found that $p = 1.8$ approximately. That result, namely, $1/\sqrt{\lambda} = 1.8N^{\frac{1}{4}}$, might be compared with Colebrook's equation (18). Within the limits of Nikuradse's diagram the formula of Blasius was not practically distinguishable from that of von Kármán. Between the $1/\sqrt{\lambda}$ -limit of that diagram and the limit of Prandtl's diagram there was no reason to attribute the departure of points from the Blasius line to any cause other than a genuine departure from smooth conditions; for it was possible to draw curves similar to those for the various r/k values, starting from the Blasius line and fitting the Colebrook points by groups. Between the limits of $1/\sqrt{\lambda} = 11$ and 12 there was one point midway between the Blasius and the von Kármán lines. No other point on Mr. Blench's diagram had such a marked deviation to the left of the von Kármán line, and, provided that that point was accurate, it afforded support to the Blasius line as the better fit to the facts. The points lay approximately on a level, a fact which was also rather hard to reconcile with the von Kármán line, but which fitted the Blasius line.

The range of $1/\sqrt{\lambda}$ from 8 to 12 was that on which to experiment for definite relative roughnesses (of small value), covering the whole range of smooth and rough for each relative roughness. That would settle finally whether the Blasius line or the von Kármán line was the better. Experiments within the Nikuradse range could not settle the matter.

That raised the following question: why, in Table IX (p. 489 §), were drawn brass and copper tubes included? Had the Author information for such tubes with $1/\sqrt{\lambda}$ greater than 8, or 9, for which the rough-boundary formula would be correct? If so, it would be of value to give it. Alternatively, was the result merely based on a small range of data which would be fitted with practical accuracy by a formula considerably different from the theoretically correct one?

Dr. Herbert Chatley referred to the Author's contention that the flow round a bend was definitely of a free vortex type. That hypothesis was developed by Dr. A. W. Brightmore* and rested on the assumption that low pressure readings inside, and high pressure readings outside, the curve indicated high and low velocities respectively, according to Bernoulli's theorem. Such an assumption was not sound, since the radial change of

†† Published by the Princeton University Press, Princeton, 1936.

§ *Ibid.*

* "Loss of Pressure in Water flowing through Straight and Curved Pipes."

Minutes of Proceedings Inst. C.E., vol. clxix (1906-07, Part III), p. 315.

pressure was not simply that due to the change of axial velocity, but depended largely on the centripetal stresses (angular rate of change of momentum). Weisbach assumed that the centrifugal effects caused the stream to separate away from the inside of the bend. In a pure flat free vortex with vertical axis the centrifugal pressures varied inversely as the square of the distance from the centre of the vortex; it was in a forced vortex that the centrifugal pressures were greatest on the outside of the curve in spite of the greater peripheral velocities there. The latter condition occurred in the impeller of a centrifugal pump. The conditions were not so simple in a circular pipe and there was an outward secondary flow due to the centrifugal pressures along the radial diameters of the pipe and, in addition, there were inward compensatory flows round the circumference of the pipe towards the centre of curvature. The centrifugal effect continued beyond the end of the bend owing to the curved path of the stream.

Dr. Brightmore also explored a 4-inch-diameter pipe with a Pitot tube at the outlet end of a bend of 4 diameters radius, and found differential head readings h^* . According to those the highest velocity did occur, in that case, at the part of the pipe nearest to the inside of the curve, but the intermediate values did not follow suit and there was a second maximum velocity near to or outside the middle of the pipe, as indicated in Table XIII.

TABLE XIII.

Discharge: gallons per minute.	Distance from inside of bend: inches.				
	$\frac{1}{4}$	$1\frac{1}{4}$	2	$2\frac{1}{4}$	$3\frac{1}{4}$
	Velocity ($\sqrt{2gh_1}$): feet per second.				
180	6.15	6.02	6.02	6.08	5.05
241	8.97	8.11	8.97	8.43	7.05
252	9.41	8.19	9.26	9.19	7.51

In fact, the velocities over the inner three-quarters of the diameter were roughly constant, so that the distribution seemed to be a compromise between a free vortex and a forced vortex. It was, however, by no means clear that the distribution at the end of the bend was the same as at the middle of the bend, and that observation might represent the return of the maximum velocity to the inside of the bend, and by no means proved that the conditions were those of a free vortex.

Mr. R. W. G. Clerke observed that the formula given for pipe-flow had, in its simple characteristics, much to recommend it to the practical engineer in the solution of his everyday problems. In practice, the flow-measurement of water with a high degree of accuracy, except where

* *Loc cit.*, p. 334.

it could be gauged by displacement or weight over a period of time (both systems which were generally confined to the laboratory), was at present not very satisfactory. That was reflected in pump and water-turbine test codes, as even with the use of specially-calibrated vane-meters, weirs and orifices, pitometers, or electro-chemical methods, a 2-per-cent. error was generally stipulated in all contracts. Even so, water-turbine efficiencies of an abnormally high character, when employing the salt-velocity method, had recently come to his notice, and suggested an even greater error than —2 per cent. in the flow-gauging. It therefore seemed that a simple easily handled formula of the type recommended by the Author had much in its favour.

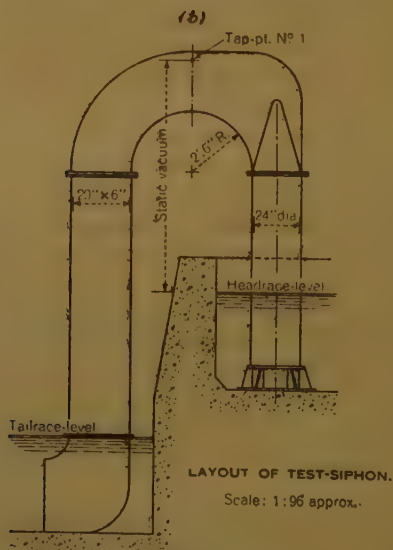
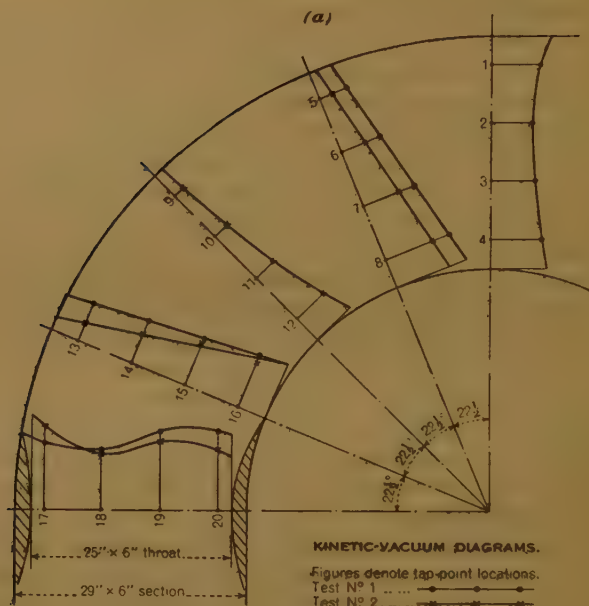
The system of reducing bends and elbows to equivalent straight lengths of pipe was one that Mr. Clerke had used for many years in waterworks practice for sluice valves, reflux valves, foot valves, and similar constrictions, based on laboratory tests by the makers. When dealing with water-supply problems with medium-sized pipes, it was the generally-recognized practice to add a percentage to the calculated friction head, depending on local conditions, to cover ageing and incrustation. Thus, again, to use a long and intricate formula, and then to add to it, appeared to have little justification.

Figs. 16 (p. 478) and *Table XIV* (p. 479) showed the results of tests carried out to ascertain the variations in the vacuum developed round the head of a siphon. Those tests were made in conjunction with the development of a siphon as a power unit to produce vacuum air-power for use in pneumatic pumping plant. A full transverse scale model in mild steel was employed, and at the tapping points indicated a mercury gauge was applied to measure the negative pressures. The figures obtained for each set of conditions were plotted for the various sections as a kinetic vacuum; that was to say, as the value over and above the static vacuum for a particular tap-point. They therefore constituted a kinetic-energy diagram, and showed remarkable uniformity for a reasonably sharp bend.

The figures for test No. 1 were for the siphon running fully primed, but even so it was found that the head was not completely filled, which accounted for the abnormally high kinetic-vacuum values at tap-points Nos. 1 and 2. Test No. 2 was carried out while air was being admitted through stream-lined aspirators at tap-points Nos. 17, 18, 19, and 20. Under those conditions the quantity of water flowing was reduced by from 40 to 45 per cent., and the crown emptied to a level between tap-points Nos. 1 and 2, while the kinetic-vacuum values dropped in proportion to the velocity of flow.

A constructive point emerged from the effect of air entering a siphon without de-priming it. It was found in practice that 12 inches of reinforced concrete had to be sealed with an asphalt layer protected by 4½ inches of masonry to render it air-tight. Whilst bearing in mind the exceptionally high vacuums employed (up to 20 feet head of water) on

Figs. 16.



DETAILS OF TEST SIPHON.

TABLE XIV.—TAP-READINGS.

Test No. 1.				Test No. 2.			
Tap number.	Gauge vacuum: feet of water.	Static vacuum: feet of water.	Kinetic vacuum: feet of water.	Tap number.	Gauge vacuum: feet of water.	Static vacuum: feet of water.	Kinetic vacuum: feet of water.
1	11.9	10.06	1.96	5	10.0	9.45	0.55
2	11.1	9.46	1.64	6	9.88	8.85	1.03
3	10.7	8.86	1.84	7	9.78	8.25	1.53
4	10.7	8.66	2.11	8	9.72	7.65	2.07
5	11.1	9.91	1.19	9	8.75	8.3	0.45
6	11.05	9.31	1.74	10	8.64	7.9	0.74
7	11.0	8.71	2.29	11	8.45	7.45	1.00
8	10.9	8.11	2.81	12	8.52	7.0	1.52
13	9.05	7.36	1.69	13	7.61	6.9	0.71
14	8.95	7.11	1.84	14	7.91	6.65	1.26
15	8.85	6.86	1.99	15	8.22	6.4	1.82
16	9.05	6.66	2.39	16	8.14	6.2	1.94
17	9.5	5.66	2.74	17	8.65	5.2	3.45
18	9.2	5.66	2.44	18	7.51	5.2	2.31
19	8.95	5.66	3.29	19	8.02	5.2	2.82
20	9.05	5.66	3.29	20	7.71	5.2	2.51

Test No. 1: Siphon fully primed, throat-velocity 7.5 feet per second approximately.

Test No. 2: Siphon partly aerated, throat-velocity 5.0 feet per second approximately.

those power siphons, it appeared equally essential to ensure absolute air-tightness for flood-discharge siphons. Steel lining was generally employed at the crown, but in long down-legs, as indicated in *Fig. 12* (p. 485 §), any air entering might materially reduce the discharge capacity of the siphon without causing it to unprime.

The Author's comments on flow in square- and circular-section bends were of particular interest. Mr. Clerke had been associated with research work on pelton wheel jets following on a 90-degree bend and on water-turbine draught-tube bends. From the test results on those jets there seemed to be no doubt that spiral flow was set up immediately after a bend in a circular pipe, even with well-proportioned bends and velocities in the neighbourhood of from 6 to 10 feet per second. All attempts to break up that spiral flow, by introducing into the bend guide vanes of varying lengths and of the same radius, had failed. The final solution was found in radial guides (as many as possible) immediately after the bend, with indications that to divide the bend into what might be termed semi-rectangular passages by two, and preferably more, radial guides would effect a reasonably effective solution in most cases.

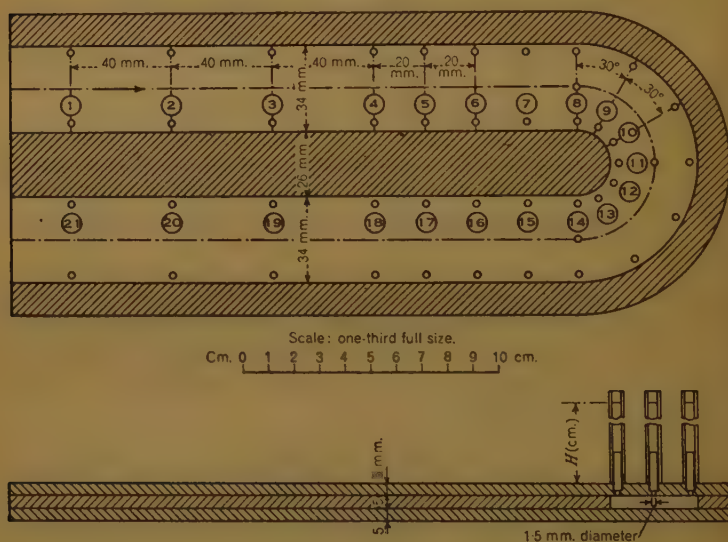
When dealing with elbowed turbine draught-tubes the problem was one of definitely damping out spiral flow imparted to the water by runner vanes, and of producing instead streamline-flow conditions before the bend.

Modern methods of draught-tube construction indicated that a very rapid flare outwards, with a proportional sharp reduction at right angles, giving a rectangular section of three to four runner diameters in width by a third of a runner diameter deep, before the necessary right angle on sharper bend, would damp out practically all spiral flow without materially affecting the efficiency of the draught tube.

There seemed, therefore, to be every indication that for bends in siphon heads, and in large pipes and conduits, rectangular sections had every advantage if spiral flow were to be avoided.

Mr. J. R. Finnicome wished to submit some further data of flow in large pipes based on actual test results.

Figs. 17.



Very little really useful and reliable data had been published on the flow round a 180-degree bend. The Author's curves shown in Fig. 5 (p. 461 §) represented, however, results which were of importance for the accurate determination of that loss in pressure head. It was not clearly stated if those curves referred to a 180-degree bend. Mr. Finnicome reproduced the test results which had been carried out with meticulous care and published in 1938 by W. Kropf*. A dimensional sketch (Figs. 17) of that model-bend indicated the points where the pressure readings were taken. The pressure distribution at two velocities along the straight

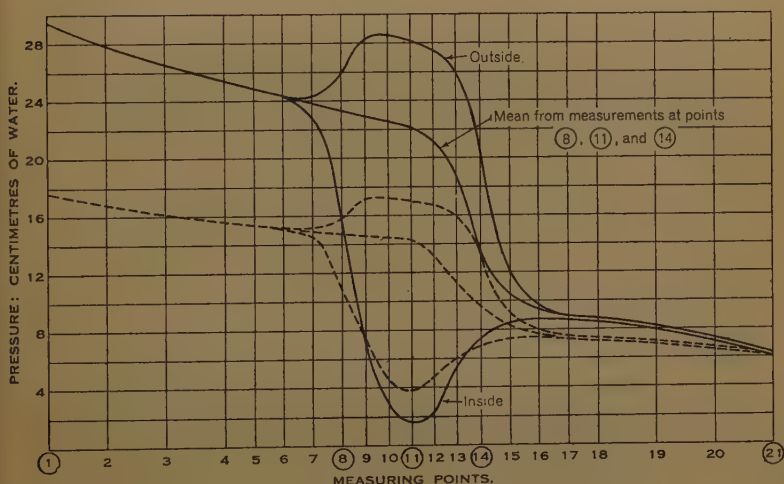
§ Ibid.

* Schweizerische Bauzeitung, vol. 112 (1938), p. 192. (15 October 1938.)

portion and around the actual bend were graphically represented in *Fig. 18*. Pressure readings were taken at twenty-one points on both the inside and outside wall and at three points on the mean path around the bend. *Fig. 18* indicated, very clearly, the rise in pressure on the outside wall of the bend, the considerable drop on the inside wall, and the head loss along the main path. From those test curves Mr. Finnicome had determined the head-loss factor around the bend based on the velocity head and the expression

$$\delta h = \xi \left(\frac{V^2}{2g} \right), \quad (7)$$

Fig. 18.



where δh denoted the pressure-head loss in feet,
 ξ " pressure-head loss factor,
 and V " average velocity in the pipe in feet per second.

The value $(\xi - 1) \times 100$ per cent. corresponded to the ordinates in the Author's *Fig. 5* (p. 461 §). The model-bend had a rectangular cross-section of 34 millimetres by 5 millimetres (1.339 inch \times 0.197 inch) and a ratio of $R/D = 0.882$. The friction factors for those bends derived from the pressure-drop curves were summarized in Table XV (p. 482). The results were also compared with the value read off the Author's curve for a square bend and that factor was tabulated in column 6 of Table XV.

Examination of Table XV showed that, for the readings from 6 to 16, the average value for ξ was equal to 1.595. The Author's curve gave,

TABLE XV.

Case.	Velocity: feet per second.	Range of readings: tap-point numbers.	ξ (Test).	ξ (Test mean).	Author's Fig. 5.
a	4.55	1 to 21	2.37	2.45	—
b	3.08	1 to 21	2.53		
a	4.55	6 to 16	1.52	1.595	1.48
b	3.08	6 to 16	1.67		
a	4.55	8 to 14	1.10	1.13	—
b	3.08	8 to 14	1.16		

for a pipe of square cross-section, a value of 1.48, which corresponded very favourably with those test results. For the difference in the pressure readings at the points 1 and 21, the loss in head was found to be 2.45 times the velocity head, whilst the average loss round the 180-degree bend, based on the readings at 8 and 14, gave a value for ξ of 1.13. Mr. Finnicome pointed out that the pressure drop around the bend commenced at the parallel portion, a certain distance before the actual change in curvature took place. In the case mentioned that length was about 1.375 times the radius of the bend.

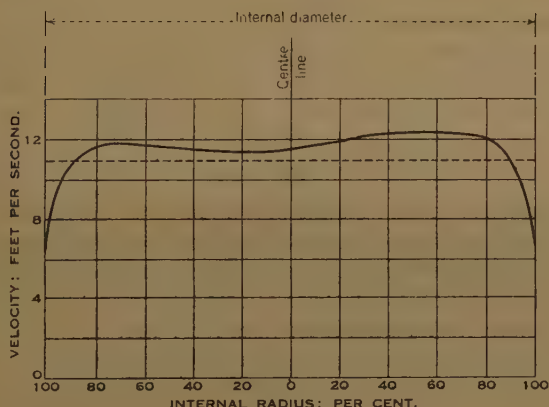
During the previous 25 years, advances had been made in the study of the velocity distribution in a circular pipe at turbulent flow. The importance of the roughness on the shape of the velocity curve was particularly emphasized by Nikuradse in his original analysis published in 1933 †. His results had evoked widespread interest, as indicated in the Paper. The Author showed the velocity curves for large pipe-lines up to 8 feet 9 inches diameter. As far back as 1911 such velocity readings had been taken on the cross-section of large pipes. *Figs. 19 (a) and (b)* showed typical curves for the 10-foot 4-inch bore pipe for the turbines at Niagara Falls. Those were published in 1913 by Dr. F. Prasil. The actual tests were made on the site. The velocities were determined by the pitot-tube method at nineteen points. That particular pipe supplied the water to a 15,000-brake-horse-power turbine and the quantities flowing were 919.4 cusecs and 930.7 cusecs in *Figs. 19 (a) and 19 (b)* respectively. The mean velocity based on the cross-sectional area of the pipe was shown by the dotted line, and amounted to 10.97 feet per second for *Figs. 19 (a)* and 11.1 feet per second for *Figs. 19 (b)*. The ratio of V_{\max}/V_{mean} was 1.13 for *Figs. 19 (a)* and 1.137 for *Figs. 19 (b)*, and V_{mean}/V_{\max} was 0.885 and 0.879 for *Figs. 19 (a) and 19 (b)* respectively. In that connexion Table XVI (p. 484) was of importance as it gave the particulars of the bore of the pipe, the inside condition at the bore with regard to roughness for the 124-inch pipe-line

† "Strömungsgesetze in Rauhen Röhren." Ver. dtsh. Ing., Forschungsheft 361. Berlin, 1933.

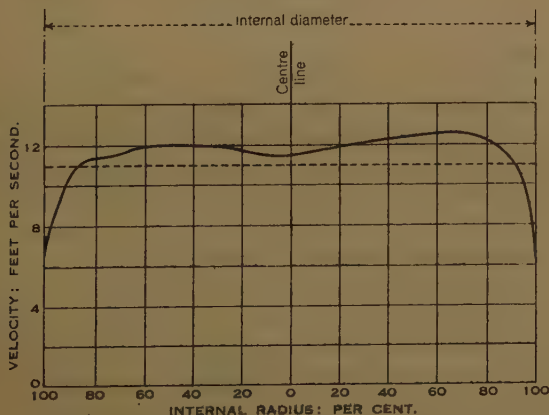
of the Niagara Falls turbines (items 1 and 2), and the pipes referred to by the Author in Figs. 2 (A) to 2 (E), Plate I (facing p. 502 §). The ratios $V_{\text{mean}}/V_{\text{max}}$ and $V_{\text{max}}/V_{\text{mean}}$, obtained from the various velocity-curves, were given in columns 6 and 7. It would be found from Table XVI that the ratio $V_{\text{max}}/V_{\text{mean}}$ ranged from 0.90 to 0.92 for new smooth-coated

Figs. 19

(a)



(b)



pipes, was 0.882 for smooth pipes, and 0.851 for rough-coated pipes. Those valuable tests on large pipes might be carefully compared with the Author's Table I (p. 460 §), which was based on Nikuradse's accurate test values for very small pipes of about 1-inch to 4-inch bore at various degrees of roughness.

In spite of the quantity of available published information it was still

TABLE XVI.—VALUES OF $V_{\text{mean}}/V_{\text{max}}$ AND $V_{\text{max}}/V_{\text{mean}}$, BASED ON THE ACTUAL VELOCITY READINGS IN LARGE PIPE-LINES.

1	2	3	4	5	6	7	8
	Description.	Condition of bore of pipe.	Bore : inches.	V_{mean} : feet per second.	$\frac{V_{\text{mean}}}{V_{\text{max}}}$.	$\frac{V_{\text{max}}}{V_{\text{mean}}}$.	Fig. No.
1	Steel	Smooth	124	10.97	1.13	0.885	19 (a)
2	"	"	124	11.10	1.137	0.879	19 (b)
3	Welded steel	New smooth coated	105	6.196	1.111	0.900	2 (A)
4	Riveted steel	" " "	80	12.483	1.099	0.910	2 (B)
5	Cast iron	" " "	72	5.645	1.085	0.922	2 (C)
6	Welded steel	Rough coating	63	5.384	1.169	0.855	2 (D)
7	" "	" "	93½	6.918	1.18	0.847	2 (E)

extremely difficult to estimate the loss of head in water pipe-lines sufficiently accurately. That was largely due to the degree of roughness, which usually could not be determined with accuracy beforehand. Only further tests on large pipe-lines could bring the estimated friction loss nearer to the actual. There were in existence more than forty well-known authoritative formulas, some having been in regular use, for special cases, for over a century. So many variable factors influenced the coefficient of friction that those formulas could be classified into seven distinct groups. The oldest of them, by R. de Prony, for rough pipes, where the friction coefficient varied only inversely with the velocity as far back as 1808. It was evident that out of those forty formulas for calculating the pressure drop in pipe-lines the Author had considered only that of Strickler. The Author was fully justified in concentrating particularly on that well-known formula, for it was the one in general use. It should, however, be pointed out that Strickler's formula was nothing else but Gaukler's fundamental formula, published in 1867. It was later brought into prominence by Manning in 1889 and again re-introduced by A. Strickler in 1923. The formula was generally accurately named in Europe as the Gaukler-Manning-Strickler formula, the latter having been largely responsible for publishing very useful and reliable data on the friction factor k or n , so comprehensively summarized by the Author in the Appendix to the Paper. Mr. Finnicome, however, emphasized that great care should be exercised in the selection of the correct friction factor n , for the accurate calculation of the actual loss of head in a straight pipe-line could be undertaken only by those with an intimate knowledge of such friction problems.

For correlating the numerous published formulas it was necessary to bring them to a common basis, and Mr. Finnicome had always applied the generally accepted basis expression :

$$\delta h = \lambda \left(\frac{V^2}{2g} \right) \left(\frac{L}{D} \right), \quad \dots \dots \dots (8)$$

where δh denoted the head loss in feet, λ the friction coefficient, V the velocity in feet per second, L the length of pipe in feet, and D the bore of pipe in feet.

Transforming formula (8) to the form of expression used by the Author (formula (4), p. 453 §), there was obtained, for the Gaukler-Manning-Strickler formula for English units, the value for

$$\lambda = 185 \cdot 8 \frac{n^2}{\sqrt[3]{D}}, \quad \dots \dots \dots (9)$$

where n denoted the roughness factor and D the bore of the pipe in feet. The value of λ could then be compared with those derived from other formulas, as Mr. Finnicome would subsequently show.

Formula (9) indicated clearly that the friction coefficient varied inversely with the cube root of the bore of the pipe and directly with the square of the roughness factor based on tests.

Mr. Finnicome had produced an interesting and useful chart, *Fig. 20* (p. 486), based on the Author's formula (4) (p. 453 §), to enable the pressure loss in large water pipe-lines due to friction to be determined quickly. By following the arrows as indicated, it was possible to read off the loss of head as a percentage of the total length of the pipe-line. The values for n refer to those given by the Author in the Appendix to the Paper.

Mr. Finnicome was rather surprised to find that the Author did not mention the valuable test data published by Mr. A. A. Barnes*, together with his empirical formula on the pressure drop of pipes lined with concrete or bitumen. To enable comparison of Barnes's value for λ with those of Gaukler-Manning-Strickler, it was necessary to bring the Barnes's formula to a common basis by equating the expression with the basic formula (8) (p. 484). Barnes gave, for the pressure loss in a pipe lined with concrete or bitumen, based on elaborate tests on numerous important aqueducts of circular cross-section in Great Britain, the following empirical formula :

$$\delta h = 0 \cdot 0003617 L \cdot \frac{V^{1 \cdot 818}}{D^{1 \cdot 309}} \quad \dots \dots \dots (10)$$

Mr. Finnicome had thus determined the value for λ in the general formula (2) and obtained :

$$\lambda = \frac{0 \cdot 02329}{D^{0 \cdot 309} \cdot V^{0 \cdot 182}} \quad \dots \dots \dots (11)$$

Besides Barnes's formula, there was also in general use for bitumen-lined pipes a formula published by Stewarts & Lloyds in 1933 which gave the loss in head as

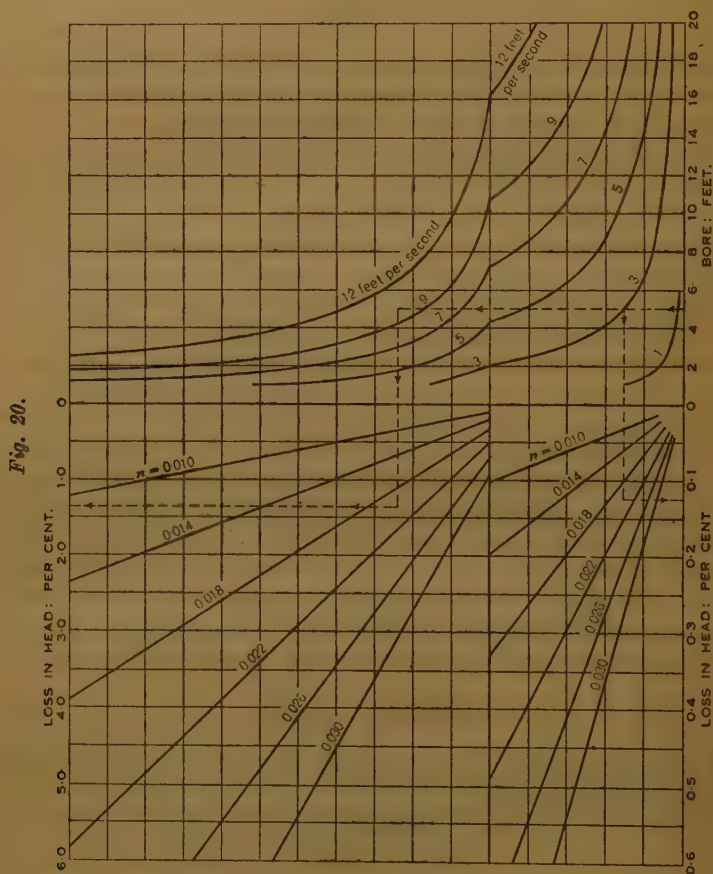
$$\delta h = 0 \cdot 00536 L \cdot \frac{V^{1 \cdot 85}}{D^{1 \cdot 15}} \quad \dots \dots \dots (12)$$

§ *Ibid.*

* "The Discharge of Pipes Lined with Concrete or Bitumen." Trans. Inst. W.E., vol. xxxviii (1933), p. 158.

Hence
$$\lambda = \frac{0.01983}{(V \cdot D)^{0.15}} \quad \dots \dots \dots (13)$$

For a fuller appreciation of the importance of Mr. Finnicome's analysis on the friction coefficient, the values for λ based on Strickler's, Barnes's, and Stewart's & Lloyds's formulas (equations (9), (11), and (13) respectively, had been plotted (*Fig. 21*) on the basis of the bore of the pipe (D) for the



roughness factors (n) of 0.009, 0.010 and 0.011 in the case of Strickler, and for velocities of 2, 4, 10 and 20 feet per second for Barnes, and Stewart's & Lloyds. The values for n were chosen to suit the curves corresponding to Barnes's value. It would be found from that graphical representation that:

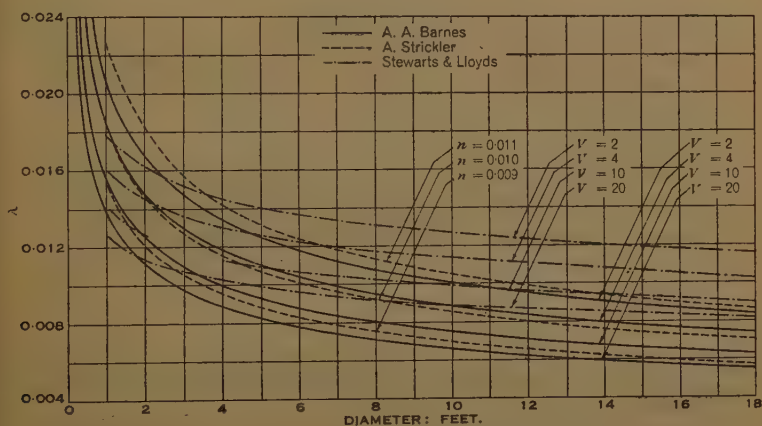
- (1) the Strickler curves were similar in shape to those of Barnes ;

(2) the Stewarts & Lloyds values were much higher than Barnes's figures ;
and (3) the Author's value for n used in the Strickler formula varied from 0.09 to 0.011 to give approximate values corresponding to Barnes's results.

That was an interesting comparison, for it showed up so clearly the great differences that existed in the values of λ of those three investigators. In the Appendix to the Paper the Author gave $n = 0.0115$ to 0.0125 for pipes lined with concrete, which, when compared with the corresponding values to suit Barnes's curves, gave from 15 to 25 per cent. higher values.

Mr. Finnicome then referred to F. C. Scobey's tests on the 18-foot-diameter concrete pipe at Ontario and to Dr. C. F. Colebrook's* calcu-

Fig. 21.



lated and corrected values for λ . The friction coefficients for the five tests at the velocities of 4, 8, 12, 10 and 20 feet per second for Barnes, Stewarts & Lloyd, and Strickler, were summarized in Table XVII and plotted in Fig. 22 (p. 488).

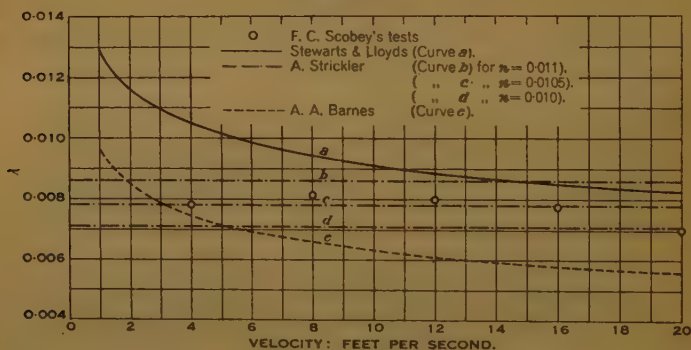
The test points (column 3) were shown in Fig. 22 by the small circles, Barnes's values were indicated by the broken line e, the Strickler lines for $n = 0.010$, 0.0105 and 0.011 were shown as chain-dotted horizontal lines b, c, d, respectively, and the Stewarts & Lloyds results, which were much higher, were represented by the full-line curve a. Using the Author's values for n of 0.0115 to 0.0125 , values for λ from 10 to 20 per cent. higher would have obtained. The difference depended largely on the degree of roughness in the pipes. Figs. 21 and 22 were typical illustrations of the

* "Turbulent Flow in Pipes, with particular reference to the Transition Region between the Smooth and Rough Pipe Laws." Journal Inst. C.E., vol. 11 (1938-39), p. 133 (February 1939).

TABLE XVII.

1	2	3	4	5	6
Test No.	Velocity: feet per second.	Test.	Friction coefficient: λ		
			Barnes.	Stewarts & Lloyds.	Strickler ($n = 0.0105$).
1	4	0.00782	0.00741	0.01044	0.00781 (constant).
2	8	0.00812	0.00654	0.00941	
3	12	0.00798	0.00607	0.00885	
4	16	0.00773	0.00576	0.00848	
5	20	0.00697	0.00554	0.00820	

Fig. 22.



relative value of such formulas for determining, accurately, the loss in head due to friction.

Professor A. H. Gibson referred to the Author's faith in the validity of the Manning formula $V = \frac{1.486}{n} R^{2/3} S^{1/2}$ for the friction loss in pipes, but did not consider it safe to assume that that formula applied also to the losses in bends, and to assume that the loss in a given bend was equivalent to that in a certain length of straight pipe of the same diameter and surface roughness as the bend, and that that length was the same whether both pipe and bend were smooth or rough. Hofmann* had carried out an extensive series of experiments in Professor Thoma's laboratory at Munich on rough and smooth 90-degree bends, and the figures in Table XVIII were taken from his results. According to those experiments it appeared that the corresponding length of straight pipe to give the same loss as the bend would not be the same for a rough as a smooth pipe, and the equivalent lengths would not be the same at 3 metres per second as at 5 metres per second.

* Trans. Munich Hyd. Inst. Bulletin 3, p. 29. Oldenbourg, Munich, 1929.

TABLE XVIII.—RATIO OF LOSS IN ROUGH AND SMOOTH 90-DEGREE BENDS.

Ratio R/D .	Velocity.	
	3 metres per second.	5 metres per second.
1	1.86	2.26
2	1.88	2.11
4	1.93	2.30
6	1.85	2.26
Ratio of resistances of rough and smooth straight pipes.	1.45	1.57

Experiments by W. Schubart *, also at Munich, on mitre bends of different deflexion angles, in rough and smooth pipes, gave results shown in part, by Table XIX. Those results were different from Hofmann's,

TABLE XIX.—RATIO OF LOSS IN ROUGH AND SMOOTH BENDS.

Angle of deflexion: degrees.	Velocity.	
	3 metres per second.	4 metres per second.
30	1.00	0.83
45	1.27	1.34
60	1.25	1.542
90	1.13	1.14
Ratio of resistance of rough and smooth pipes	1.35	1.45

but again showed that the appropriate length of straight pipe varied with the roughness and the velocity. The experiments showed, moreover, that the loss in a rough bend might, in some circumstances, actually be less than in a smooth bend.

In a Paper by K. H. Beig †, experiments were described on a series of 4-inch 90-degree bends of various radii. In those pipes, which were of steel, the bend losses were, within the errors of measurement, all proportional to v^2 over a wide range of the Reynolds number. On the other hand, the value of the frictional coefficient for the straight pipe fell steadily with increasing velocities, so that there again no single length of straight pipe would give the same loss as a bend, at all velocities.

In all those cases, if curves of the loss in the straight pipes and in the bends were plotted on a basis of Reynolds numbers, it would be seen that

* Trans. Munich Hyd. Inst. Bulletin 3, p. 81. Oldenbourg, Munich, 1929.

† Research Paper R.P. 1110. U.S. Bureau of Standards, Journal of Research, vol. 21 (1938), p. 1 (July 1938).

the law of change of resistance with velocity was different for the straight pipe and the bend, and if that were so it followed that the law of change of resistance with diameter would also be different for the two cases.

Beig discussed, in his Paper †, the results of other workers in the same field and gave curves showing the values of bend coefficients of different radii, as determined by various investigators as functions of the wall roughness. According to those curves the bend coefficient was proportional to the x th power of the absolute roughness, where x had values of 0.34, 0.40, 0.55 for values of R/D of 1.0, 2.0 and 4.0 respectively. According to the Author the index should, in each case, have been equal to 0.33, but for easy bends it was apparently much greater than that.

The Author showed (p. 465 §) how, by using his formula to correct for friction losses, he could bring the results for the small Manchester siphon model into agreement with the large model, and, indeed, used that fact as a verification of the validity of the formula. If that correction could be made there would appear to be no point in using the larger and more expensive models, but the Author apparently did not agree with that point of view, since he stated (p. 482 §) that the minimum diameter of passage should not be less than 2 inches, whereas in the small Manchester model that diameter was only 1 inch. The Author stated also (p. 456 §) that the formula on which the friction correction was based was not applicable to pipes in which the hydraulic radius was less than 1.5 inch or in which the walls were very smooth. As each of the Manchester models had very smooth walls and as the smaller had a hydraulic radius of only 0.375 inch, the point did arise as to how far the Author's use of the formula, in deducing a friction correction, was justified.

It would be interesting to know the experimental evidence for the statement (p. 481 §) that where variation of cross section caused changes of velocity, the loss of head varied as $V^{2.67}$. All the evidence available to Professor Gibson indicate that in the case of an increasing section the loss varied as $(V - v)^2$, where V denoted the initial and v the final velocity.

The statements on p. 460 § and elsewhere, that the pressure head at the side of a straight pipe was greater than the mean pressure at that section by an amount which might be as large as $0.9 v^2/2g$, called for some comment, as it was quite contrary to the results of observation. Actually, there was no sensible difference between the pressure at the walls and in the body of the fluid, except where the fluid had a whirling motion giving rise to centrifugal pressure. That was evident from the fact that all Venturi meters, irrespective of the ratio of diameters at entrance and at throat, and irrespective of size, had substantially the same discharge coefficients.

Mr. Gerald Lacey, of Lucknow, thought that it was preferable to deal in terms of types of flow rather than types of pipe. Using that method

† *Loc cit.*

§ *Ibid.*

of classification there were four stages of flow:—streamline, smooth turbulent, transitional turbulent, and fully turbulent.

If the Manning-Strickler relation

$$V \propto (R/k)^{1/3} (gRS)^{1/3}$$

were restricted to the fully turbulent stage there was no violation of any dimensional rule or relation. The equation was an approximation to the Prandtl-von Kármán rough-pipe law, which had, as a background, a logarithmic velocity distribution in the pipe; if that distribution of velocities were accepted, empirical formulas of the exponential type could never be anything but approximations.

Dr. C. F. Colebrook had demonstrated that, for values of R/k from 2.5 to 50, the Forchheimer equation, $V \propto R^{0.7} . S^{0.5}$ applied, and that, throughout the great range of values of R/k from 50 to 10,000, the equation $V \propto R^{0.61} . S^{0.50}$ applied. It was possible, therefore, to find a very large, fully-turbulent intermediate range to which the Manning equation would apply. It was true that the accuracy, assuming the Prandtl-von Kármán relation to be rigidly correct, would be limited to an error of $2\frac{1}{2}$ per cent., but that was well within the limits of observation error.

If, in the place of a logarithmic velocity distribution, an alternative theory, such as that of Mr. Thomas Blench, were considered, based on vorticity, then it was possible to put forward an exponential equation, such as

$$V \propto (R/k)^{1/3} (gRS)^{1/3},$$

which had a physical background as defensible as that of the Prandtl-von Kármán relation. If it were granted that the Prandtl-von Kármán assumption in respect of velocity distribution was fundamental and established beyond doubt, all exponential equations were immediately subject to the charge that they were merely empirical. In respect of pipes the final theory had not of necessity been formulated; whilst in respect of open channels, particularly in open channels in alluvium which generated their own boundaries, there was nothing to show that the Prandtl-von Kármán theory was not merely a close fitting to observed facts.

The Author, it was clear, had made good his point, that in the fully turbulent stage the Manning-Strickler equation was a close approximation to the Prandtl-von Kármán theory. Mr. Lacey, however, was far from certain that the Author had, in fact, restricted himself to the fully turbulent stage. Dr. Colebrook had pointed out* that good commercial drawn-brass pipes could be regarded as hydraulically smooth for all ordinary velocities of flow. The Author, in his reply to the Discussion, had stated that he would not attempt to give any results for "conditions which came partly within the viscosity conditions." That being so, Mr. Lacey found

* "Turbulent Flow in Pipes, with particular reference to the Transition Region between the Rough and Smooth Pipe Laws." *Journal Inst. C.E.*, vol. 11 (1938-39), p. 143 (February 1939).

it difficult to understand why the Author, in the Appendix, Table IX (p. 489), had quoted values of the coefficient of roughness for wood-staved pipes, and for drawn-brass and copper tubes. It was certainly doubtful whether data existed for drawn-brass tubes in the fully turbulent stage. Table IX, from its range, assigned to the empirical Manning equation a universality which the Author, in the Paper itself, disclaimed. The true explanation appeared to lie in the fact that, quite fortuitously, the Manning equation could be employed as an approximation, not only in the fully turbulent stage, but also in the smooth turbulent stage.

Dr. Chatley had referred to the fact that the Manning equation, the Forchheimer equation, and Mr. Lacey's equation all violated a certain rule or relation between the exponents. It was necessary to point out that the rule or relation had no bearing whatever on equations for fully turbulent flow. It was of interest to examine the rule and to discover how it came about that the flow of water through smooth pipes could be fitted to an equation such as that of Manning.

Adopting the notation of the Author and writing the general equation

$$V \propto R^a S^b,$$

the exponents a and b were connected by the relation

$$a = 3b - 1, \text{ or } b = \frac{a + 1}{3}.$$

The underlying assumption was that, in both streamline flow and smooth turbulent flow, the frictional coefficient was a function of the Reynolds number, and of nothing else. That, at least, was an established fact.

It was of interest to note that Mr. Hellstrom had found that for smooth wooden pipes the power of S was 0.55. That would be entirely consistent with a power of R of 0.65. The fact, however, could not entirely justify the application of the Manning equation to the smooth turbulent range; actually the equation did apply (without any physical background to support it) by a fortunate coincidence, and because the great mass of data were for the flow of water. It appeared to Mr. Lacey that a stage was now within reach at which it should be possible to assign to certain empirical equations for flow in pipes, the limitations of their application. The application of empirical pipe equations to flow outside their range or to open channels was a proceeding of doubtful validity.

Mr. J. M. Lacey observed that the Paper dealt chiefly with the resistance in pipe-bends and siphons: it was of interest to mention two earlier Papers on the resistance in bends*.

On p. 453 § the Author gave what he termed the "foundation-formula"

* C. W. L. Alexander, "The Resistance offered to the Flow of Water in Pipes by Bends and Flows." Minutes of Proceedings Inst. C.E., vol. clx (1904-5, Part I), p. 341.

A. W. Brightmore, "Loss of Pressure in Water flowing through Straight and Curved Pipes." *Ibid.*, vol. clxix (1906-7), Part III, p. 315.

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 11 (1938-39), p. 451 (April 1939)).—SEC. INST. C.E.

recommended by Dr. A. Strickler with applications to rivers, canals, and closed conduits, which expressed in foot-second units became

$$V = \frac{1.486}{n} R^{\frac{2}{3}} S^{\frac{1}{2}}.$$

Values of n were given in Table IX (p. 489 §), and its value correlated to absolute roughness or size of protuberance in Table X (p. 490 §). Later investigations seemed to show, however, that the density of the protuberances was a factor to be considered ‡.

In Table X (p. 490 §) the Author defined "absolute roughness" as that which might be visualized as the average diameter of projecting rounded grains forming the roughness of a uniform surface. Had the correlated values of n and "absolute roughness" been determined by actual experiment? A value of $n = 0.025$, for a channel-bed consisting of rounded pebbles 3.3 inches in diameter, seemed to be low. From numerous gaugings of canals in India the value of n for smaller channels was found to average 0.0225, and for large channels it averaged 0.02. That was for regular channels in earth with smooth sides and bed. Where canals or channels had beds of sand the size of sand-grain was a small fraction of an inch, and not any size approaching that between 3.3 inches and 0.88 inch.

The formula given by the Author might be applicable for pipes, and perhaps for trapezoidal canals with uniform depth and smooth flow. In pipes all sizes were similar, and the roughness in each pipe was uniform, so that the diameter or hydraulic mean radius could be assessed as a characteristic length or parameter. The same might be said of wide trapezoidal canals of uniform depth, when the depth was equal, or nearly equal, to the hydraulic mean radius. When natural channels were considered, however, the hydraulic mean radius was no longer a parameter; channels of various shapes might have the same values for the hydraulic mean radius, whether wide and shallow, or comparatively narrow and deep. Experiment had shown that the coefficient of roughness decreased as the depth increased.

In uniform flow when there was neither acceleration nor retardation, the effective force per unit width of channel was wid , where w denoted the weight of water per unit volume, i the slope of the water surface, and d the depth, and it seemed necessary, in any formulas applicable to natural channels of irregular shapes, to divide the transverse water area into sections of more or less constant depth, in computing the discharge.

When, however, streams were transporting solid matter, either in

§ *Ibid.*

‡ C. F. Colebrook and C. M. White, "Experiments with Fluid Friction in Roughened Pipes." *Proc. Roy. Soc. (A)*, vol. 161 (1937), p. 367.

H. Schlichting, "Ein neues Verfahren zur Messung des Strömungswiderstandes von rauhen Wänden." *Werft Reederei Hafen*, vol. 17 (1936), p. 99.

suspension or by traction, conditions as to density, viscosity, and weight per unit volume were altered, and it was no longer possible to treat the flowing matter as homogeneous.

Mr. Arnold Maude observed that, in the Discussion, Mr. E. R. Howland had challenged the Author's contention that, with flow in straight pipes there was an excess side pressure over the mean pressure. If such an excess existed and could reach as much as 0.9 of the velocity head, its neglect was a serious matter both in water-turbine and pump tests, particularly when the total head was small. It appeared difficult, however, to reconcile the idea of the existence of such an excess, particularly when steady flow conditions had been attained, with the basic laws of dynamics. No particle of matter could be subjected to an unbalanced force without suffering acceleration in the direction of action of such force, and therefore an excess side pressure was bound to involve a centripetal acceleration of the water particles near the walls; since, however, the flow-conditions were steady, any such acceleration was bound to be balanced by a corresponding outward acceleration of particles near the centre, which could only happen if there were an excess centre pressure over the side pressure.

The only acceleration towards the centre compatible with uniform motion was in the case of rotation; if the water in the pipe were following a steeply helical path, excess side pressure could, and did, exist, but that did not appear to be the condition contemplated by the Author in Table I (p. 460 §).

Mr. P. W. Seewer considered that the Author's methods of calculating losses in straight pipes and bends, and his considerations on vortex flow, led to a solution of siphon problems which were simple and ingenious.

His formula for friction losses in straight pipe had the advantage of enabling coefficients of the Chezy type to be determined without difficulty. It was derived by Dr. A. Strickler, but it would be recognized as being identical with the older formula known as Manning's, and it was satisfactory to find that two independent investigators had arrived at the same result in such a complex field of research.

The losses were proportional to the square of the mean velocity, which was said to be a condition of completely turbulent flow with the effect of roughness being fully developed. Further on in the Paper, however, it was stated, and shown in the traverses, that velocity-curves in large pipes did not always show the typical form for fully developed roughness. It might then be inferred that the losses in large pipes should be proportional to a lower power of the velocity than 2, but evidently the Author retained that index for such pipes. The index in some other formulas was lower; for example, Messrs. Williams and Hazen made it 1.85.

On p. 460 § it was stated that the pressure read at the side of a pipe was always greater than the mean pressure, and that that point was worthy of careful attention in the analysis of turbine-efficiency tests.

Usually the gauge-reading was accepted as giving the pressure component of the net head, but if the efficiency calculated from that happened to fall short of the guarantee, any assistance which could be obtained on account of that statement would be appreciated. For example, the velocity-head at the entrance to a turbine-spiral designed for a low or medium head might be as much as 4 per cent. of the net head, and so, by profiting from the corrections for pressure given in Table I (p. 460 §), the improvement in the efficiency of a large machine might amount to 2 per cent. at full load.

The Author's remarks on the functions and requirements of the surge-tank were well founded. It was, indeed, the case that a surge fluctuation produced by a change of load when the station was on governor control was quickly reduced to steady flow by the automatic action of a well-designed governor, and it was at first sight a remarkable feature of such a mechanism that it was able to control an enormous mass of water so effectively. It would be apparent that the governor had to be suited to the local conditions of the plant; on the governors in the various stations of the Galloway scheme, to which the Author referred, that was done by means of a very simple adjustment.

The Thoma formula was useful because it gave an idea of the size below which even a small oscillation due to a regulating movement would be amplified. It did not, however, indicate the minimum size of tank that could be adopted with satisfactory results, but rather a size which, having regard to the assumptions made in the derivation of the formula, it was wise to exceed with an ample margin to ensure that disturbances would rapidly die out. The margin of 25 per cent. on the minimum diameter was adequate for the surge-tanks of the Galloway scheme, provided that that diameter was determined from the friction losses in the aqueduct alone, to which the coefficient C in the Paper referred. In some other statements of the formula, C took account of the velocity-head as well, and, if that were appreciable (as it sometimes was) the margin on the diameter of the tank calculated from the larger value of C would be very much more than 25 per cent. Generally speaking, the size finally adopted was determined more by the allowable upward and downward surges accompanying the rejection and assumption of load respectively, and by their influence on the frequency of the system.

The Author's formula for pipe-friction might be used to obtain an expression for predicting the efficiency of a full-scale turbine from that of a geometrically similar model. A comparison of the losses in the two machines led to the following expression :

$$E = E_m \left\{ 1 - \left(1 - \frac{e}{e_m} \right) \cdot \left(\frac{N}{n} \right)^2 \cdot \left(\frac{d}{D} \right)^{0.333} \right\},$$

where E and e denoted the efficiencies of the full-scale and model-machines respectively, E_m and e_m the mechanical efficiencies of the full-scale and

model-machines respectively, $\frac{N}{n}$ the ratio of the roughness-coefficients in full-scale and in model, and $\frac{d}{D}$ the scale-ratio of the model to the full-scale machine, in the present case presented as the ratio of the runner diameters.

If the ratio of roughness-coefficients were made equal to unity, the expression closely resembled the formula of Fromm, which was

$$E = E_m \left\{ 1 - \left(1 - \frac{e}{e_m} \right) \cdot \left(\frac{d}{D} \right)^{0.314} \right\}.$$

According to published information, Fromm's expression when applied to two different models of the Lawaczeck turbine of Lilla Edet, gave efficiencies that agreed very closely with what was obtained on the full-scale machine. (The scale-ratios were $\frac{1}{13}$ and $\frac{1}{6}$.)

There were other expressions connecting full-scale and model-efficiencies, notably those of Camerer and Moody. They were derived from pipe-friction formulas, and were usually presented without the symbols for mechanical efficiency. Each was no doubt applicable to the conditions covered by the pipe formula from which it was derived. Referring to the expression due to Fromm, it was said that the resistance depended only on the relative roughness of the surfaces for turbine-passages having either plate vanes and cast-iron naves and rims, or made of cast iron throughout, and for high Reynolds numbers. Similarly, since the Author's friction formula was for use when the effect of roughness was completely developed, the expression derived from it should apply only to such conditions.

The Author, in reply, noted that losses in bends were referred to by Mr. Addison and Professor Gibson and further information was adduced. Mr. Addison's experiments showed greater resistance in rough cast-iron bends of square section than in smooth machined-brass bends of the same section, and indicated that roughness was an important factor. The Paper was particularly concerned with bends for syphons, and the Author's investigation showed that for usual arrangements the resistances of the separate bends could be added together. The investigation did not extend to combinations of interfering bends.

In laboratory tests involving both smooth and rough conditions there was very great difficulty in making comparisons if part of the investigation fell within a region of smooth-pipe law and another part within the region of rough-pipe (V^2) law. In a small straight smooth pipe the flow might be in the region of smooth-pipe law, whereas in a corresponding bend, owing to the much greater resistance, the rough-pipe law, or an approach thereto, would probably apply. With a rough pipe and a rough-pipe bend the flow in both might follow the rough (V^2) law. As the resistance coeffi-

cient in the smooth-pipe region was greater than in the rough-pipe region, the relative resistance of a rough straight pipe to a smooth straight pipe should be less than the relative resistance of the corresponding bends. Mr. Addison stated that, for his experiments, "the ratio between the losses [in bends] was greater even than the ratio between the losses in comparable straight lengths of pipe." That was really what should be expected, as his machined straight pipes would come within the smooth-law region. The Author had not had an opportunity of studying the Papers referred to by Professor Gibson. Tables XVIII and XIX (p. 489, *ante*), however, indicated clearly that the smooth-pipe law affected the comparison of resistances of the rough and smooth straight pipes (lower line of each Table). If the same law had applied to each pipe, then the same ratio would have been found at 3 and 5 metres per second in Table XVIII and another constant ratio at 3 and 4 metres per second in Table XIX. If any useful comparison with the Author's results were to be made, then the effect of the smooth-pipe conditions should be eliminated or allowed for. The particulars, as abstracted by Professor Gibson, were not sufficiently detailed to enable analysis to be made. The Author considered that he was justified in applying Mr. Davies's results on teak-wood bends to the large-pipe scale where the V^2 law was found to apply almost exclusively, since that law was found to apply both to the pipes and to the bends of his (Mr. Davies's) experiments, and the surface roughness could be relied upon as being practically the same in both. In bends of circular section there was great practical difficulty in ensuring that the roughness coefficient of the bend surface (including irregularities of jointing and manufacture) was the same as the roughness coefficient of the straight pipe used for comparison. The Author's reasons for considering that turbulent flow would occur in Professor Gibson's small siphon-models were stated clearly on p. 465 §. The special conditions conducive to turbulent flow (high velocity, sharp bends) in those small siphon-models might not apply to other classes of experiment, and in general the Author considered that a minimum diameter of 2 inches was desirable where bends were concerned. In the case of straight smooth pipes, larger sizes and velocities were required if the smooth-law region was to be avoided; an indication of the limits based on Nikuradse's experiments was given in Table XX*, p. 498.

Mr. Addison, Professor Gibson, and Mr. Maude referred to the point raised by Mr. Howland in the Discussion as to the uniformity of pressure across the whole section of a pipe. The Author was in entire agreement with Mr. Addison's suggestion that some definite experiments should be made, and he appreciated the difficulties attendant on determining exact pressures in the body of a flowing stream. Professor Gibson's and Mr. Maude's deductions from theoretical considerations did not appear

§ *Ibid.*

* This Table formed part of the MS., but was omitted from the abridged Paper published in the Journal.—J. W.

TABLE XX.—APPROXIMATE MINIMUM VELOCITIES IN SMALL STRAIGHT PIPES FOR
APPLICATION OF FORMULA $S = \frac{V^2 n^2}{2.2R^{1.33}}$ (FOOT-SECOND UNITS).

(Based on Nikuradse's Experiments.)

Roughness coefficient, "n"	Diameter of pipes.			
	1 inch.	2 inch.	4 inch.	6 inch.
	Minimum velocities: feet per second.			
0.013	4	3	2	1½
0.012	6	5	4	3
0.011	8	7	6	5
0.010	10	9	8	7

to be conclusive. If it were the case that Bernouilli's law applied to flow in a bend and to flow in a Venturi contraction, but not to steady flow in straight pipes leading to such parts, it would be useful to engineers to have conclusive evidence and to know the facts and the reasons. Table I (p. 460 §) was prepared on the basis that Bernouilli's law applied to steady flow in pipes.

Mr. Blench made an interesting comparison between his formula for turbulent flow, $V = (\text{absolute constant}) \left(\frac{R}{k}\right)^{\frac{1}{4}} \sqrt{gRS}$, and the Manning formula adopted by the Author, and pointed out their similarity, the difference being that the exponent of $\frac{R}{k}$ in the latter formula was $\frac{1}{6}$ instead of $\frac{1}{4}$. The Author agreed that both formulas were dimensionally correct and that over moderate ranges of R the results given by the two formulas would not be widely different provided that the absolute constant were suitably determined for each. The Author referred Mr. Blench and Mr. J. M. Lacey to Table XI, p. 502 §, in the reply to the discussion, for the correlation of n and k in the V^2 -law ranges of Nikuradse's careful experiments. The values of k were measured for the artificial uniform sand-grain surfaces used and the values of n were calculated by the Author by means of the Manning formula, using the recorded velocities, gradients, and diameters in those parts of the investigation where the V^2 law applied. The Table indicated the close degree of correspondence between the Manning formula and the experimental results. Mr. Blench's formula with a term containing $\left(\frac{R}{k}\right)^{\frac{1}{4}}$ could not give such close correspondence.

Referring to Dr. Chatley's further remarks on vortex flow, the Author maintained that a tendency towards free-vortex flow, which was all

that was claimed in the Paper, had been amply proved by various experimenters who had made actual velocity traverses. At the centre of a 180-degree bend the actual conditions corresponded closely with free-vortex flow, and that was generally accepted as a fundamental condition to be provided for in the sharp crown bends of syphons, where Bernoulli's law applied closely. The Author agreed with Dr. Chatley that, in a free vortex with vertical axis, the centrifugal pressure produced by a single curving filament varied inversely as the square of its distance from the axis. Since, however, the pressures produced by successive filaments in proceeding from the axis outwards were cumulative, the maximum total pressure in the stream was at the outside of the bend. The Author did not contend that any approach to free-vortex conditions occurred in the transition stage at the outlet end of a bend, and that should be clear from his description of *Fig. 7*, p. 462 §.

The pressure records produced by Mr. Clerke (*Figs. 16*, p. 478, *ante*) were interesting and indicated a tendency towards free-vortex flow at the middle of the bend, in spite of unusual entrance and exit arrangements. His experience with spiral flow, produced at the outlets of round-pipe bends, was consistent with a general tendency, which made the determination of true losses more difficult in round-pipe bends than in square-pipe bends.

Mr. Finnicome's pressure diagram, *Fig. 18*, relating to the bend of *Figs. 17*, indicated pressure distributions which also were consistent with free-vortex flow. They might be compared with the Author's indications from calculations given for crown bends of syphons in *Figs. 9* and *10*, Plate 1 (facing p. 502 §). The resistance of the bend, *Figs. 17*, was in good correspondence with Mr. Davies's results for square pipes when account was taken of the variations of R/D and of the hydraulic mean radius.

The Author's curves in *Fig. 5* (p. 461 §) were for theoretical free-vortex flow. That was approximately attained at the middle of a 180-degree bend, but might be only partially attained in a bend of small angle.

The velocity curves (*Figs. 19*), were typical of a number which had come to the Author's notice for cases where the length of travel in a straight pipe had not been sufficient to develop the velocity curve corresponding to steady flow. The length of travel for the curves of *Figs. 19* was not stated, but most of the pipes for the power-stations at Niagara Falls were comparatively short.

The chart, *Fig. 20*, submitted by Mr. Finnicome, should be very convenient for determining the loss in large pipe-lines from the formula

$$S = \frac{V^2 n^2}{2 \cdot 2 R} 1 \cdot 33.$$

The Author did not include in Table IX of the Appendix any values for bitumen-lined pipes, and was well aware that there was a range of low

velocities in smooth pipes of the smaller sizes where the V^2 law did not apply. That condition might be expected particularly in asbestos pipes, bitumen-lined pipes, and spun-concrete pipes. It should be particularly noted that in the smooth-pipe-law region (small pipes, low velocities) the coefficient of resistance was greater than in the rough-pipe-law region. In practical engineering calculations for large pipes the Author would be inclined to take no value of n below 0.11. In model-tests the actual ascertained value should, of course, be used. The Author gave the range of values $n = 0.010$ to $n = 0.0115$ for large machine-made concrete pipes, not $n = 0.0115$ to $n = 0.0125$ as stated by Mr. Finnicome. The latter figures were given for concrete-lined tunnels and aqueducts from steel forms, that were made in situ. It would be noted that the range $n = 0.010$ to $n = 0.0115$ lay intermediately between the extremes of the Barnes and Stewarts and Lloyds curves shown in *Fig. 21*.

Fig. 22 was important, for it demonstrated that the flow in the very smooth Ontario tunnel, referred to by Dr. C. F. Colebrook,* followed practically the V^2 law with coefficient $n = 0.0105$ in the Manning formula, and it was clear that any smooth-pipe law extended to apply to that tunnel would give results very wide of the mark. That agreed with the Author's experience in other tunnels having comparable flow conditions, the surface being rather rougher but the diameter smaller. For a tunnel of 11 feet nominal diameter, tests showed that the V^2 law applied down to a velocity of 2.7 feet per second, below which value no tests were made. The value of n was about 0.012.

With reference to Mr. Gerald Lacey's remarks, the trend of the Manning formula, $V \propto R^{2/3} S^{1/2}$, was intermediate between those of the two formulas attributed to Dr. Colebrook for different ranges of $\frac{R}{k}$, $V \propto R^{0.7} S^{0.5}$ and $V \propto R^{0.61} S^{0.5}$. The results would not be widely different over considerable ranges of R . The smaller values of n were given in Table IX because they were particularly pertinent to siphon-model tests, where in most cases turbulent flow was produced by the sharp bends. The values of $n = 0.0084$ for smooth teak, and $n = 0.0064$ for smooth varnish on smoothed wood, were determined for turbulent flow conditions in small models which included bends, and values down to $n = 0.0082$ were found in straight pipes of 1 inch to 4 inches diameter from Nikuradse's experiments. Turbulent-flow conditions could readily be found in small drawn-brass models, including sharp bends, and also in straight pipes at a sufficiently high velocity. Table XX (p. 498, *ante*) was a guide to the limits for small straight pipes. That explanation might enable Mr. Lacey to appreciate that there was a field for the application of the Manning formula even in

* "Turbulent Flow in Pipes, with particular reference to the Transition Region between the Smooth and Rough Pipe Laws." *Journal Inst. C.E.*, vol. 11 (1938-39), p. 143 (February 1939).

pipes as small as 1 inch diameter and at the lowest values of n given in the Appendix, Table IX.

On the point of correlation of n and k referred to by Mr. J. M. Lacey, comparison of Table X in the Paper with Table XI in the Discussion should show that over the range from $n = 0.0082$ to $n = 0.013$, the values of n were closely correct for actually-measured values of k . Over the range of those experiments it was found that k was proportional to n^6 and that relation had been used to extend the Table upwards and downwards. There was nothing inconsistent with a value of k of 3.3 inches for a regular channel-bed of pebbles and the same value for an indifferant canal in earth or sand, where irregularities of surface were always present and were predominant in causing the resistance. A canal in earth or sand conveying flowing water and having smooth sides and bed was, in the Author's experience, never found, and the sand-grain size was not then a measure of k .

Mr. Lacey's observations in his last paragraph were pertinent to all cases where the surfaces were erodible. In a number of cases in rivers, it had been found that, up to a certain velocity, the bed was stable and the value of n was sensibly constant. With increasing velocity the bed material began to travel, the resistance increased, and the value of n rose considerably, but in very irregular fashion.

Paper No. 5169.

"The Strengthening and Final Testing of the Pressure Tunnel for the Water-Supply of Sydney, N.S.W." †

By SAMUEL THOMAS FARNSWORTH, B.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Mr. H. H. Dare, of Roseville, N.S.W., observed that, for the design of the lower section, 15,345 feet long, it was assumed that there would be a head of 405 feet below the top water-level of Pott's Hill reservoir; that would be increased to 504 feet when the proposed elevated service reservoir was erected in the future. It had been estimated, however, that there would then be a balancing hydrostatic pressure equal to the head of the ground-water, which, for purposes of design, it was assumed would reduce the net head to 273 feet ultimately.

† Journal Inst. C.E., vol. 11 (1938-39), p. 561 (April 1939).

Testing in stages was decided upon by the Committee, of which Mr. Dare was a member; the tunnel was to be filled gradually, and the head at each stage maintained for about a week, during which time measurements were to be made of the quantity of water necessary to maintain the prescribed head.

When the final test under 400 feet head was made the ground-water pressure had only risen to $23\frac{1}{2}$ lb. per square inch, so that the net unbalanced head on the lower section was 330 feet, compared with 273 feet for which the lining was designed.

The corresponding heads on the upper section, 36,425 feet long, were about 120 feet less than on the lower section, whilst the ground-water pressure during tests was zero. No distortion under the maximum test head was observed in either section.

The appearance of the bituminous lining did not alter between the date of the final inspection by the Committee and the subsequent inspection, after the tunnel had been in commission for nearly 6 months, except that the blisters in the small air section of the roof, observed during the first inspection, had disappeared. It was reported, however, by Mr. W. D. Goudie, Chief Maintenance Engineer, that the lining was considerably harder than when originally installed, due probably to the high pressure to which it had been subjected during the intervening period.

The bituminous lining was spun hot * by the centrifugal process on to the steel pipes, which had been heated to a temperature of about 105° F. The Author had referred to the evidence of the blistering and subsequent laboratory experiments as indicating a certain porosity in the bituminous lining, not previously anticipated. The Sydney water-supply was drawn from a clean sandstone catchment, and had a pH value of 6.7. There was no evidence to show what its action, if any, might be on the bituminous lining over a period of years under the heads obtaining in the pressure tunnel, but there seemed no reason to depart from the original opinion formed by the Committee that, even if not everlasting, the lining would protect the steel pipes for many years.

A pleasing feature of the rehabilitation was the abnormally low leakage over the 10 miles of tunnel observed when under test. The leakage figures quoted by the Author compared very favourably with those recorded elsewhere.

Mr. A. R. Ford, of West Maitland, N.S.W., made special reference to the marked reduction in the capacity of the pipe caused by the internal projections. Those projections, although well faired off, caused large eddies which considerably reduced the effective diameter.

The designers used the Williams-Hazen formula, but it would be interesting to state the results in terms of the Kutter formula. Taking the

* G. Haskins, "The Construction, Testing, and Strengthening of a Pressure-Tunnel for the Water-Supply of Sydney, N.S.W." Minutes of Proceedings Inst. C.E. vol. 234 (1931-32, Part 2), p. 25.

maximum flow during the test to be 47,500,000 gallons per day, or 88 cusecs, C in the Chezy formula was found to be 97.5, giving a value of n in Kutter's formula of approximately 0.0168, which was admittedly very high. Experiments described by Mr. S. W. Jones, B.Sc., M. Inst. C.E.*, showed that for 15-inch-diameter butt-welded pipes, thinly coated with a tar and bitumen compound, the value of n for a similar velocity was approximately 0.017, reducing as the velocity increased; the corresponding value of C in the Williams-Hazen formula was 138, increasing with higher velocities. That indicated the large difference in capacity caused by the joints; even if the calculations took account of the reduced area, n worked out to be approximately 0.016. It might be concluded that the contractions of the joints gave an increase in the value of n from about 0.011 to 0.017, or a decrease in the value of C in the Williams-Hazen formula from 138 to 96. Those results indicated a loss of capacity of at least 30 per cent., based on the maximum area of the pipe, compared with a loss of area at the constrictions of only 5 per cent. To put it in another way, a pipe with a smooth bore of just less than 7 feet would have the same capacity as an 8-foot 3-inch diameter pipe with constrictions.

It would be interesting to know if the actual test results, when plotted over the full range, agreed more nearly with the Williams-Hazen formula or with the Kutter formula. On drawing graphs for those two formulas and also for the Manning formula, and making them all coincide at the point corresponding to 88 cusecs, it was found that the Manning and Kutter curves were almost identical, whilst the Williams-Hazen curve gave smaller values of discharge than the other two for smaller heads; for larger heads than 7.32 feet, the Williams-Hazen formula indicated a considerably greater flow than did the other two formulas. At 30 feet friction head the approximate discharges indicated were:

Manning formula	:	179 cusecs.
Kutter formula	181 cusecs.
Williams-Hazen formula	188 cusecs.

Mr. Ford thought that there was room for more research into the nature of the eddies set up by restrictions in pipes.

Mr. H. J. F. Gourley was surprised that the effect of the streamlined shape of the asphalt lining at the joints, reducing the clear diameter by about 2 per cent., was to reduce the friction coefficient by 20 per cent. from 120 to 96.

The Author's explanation of the occurrence of blisters on the soffit of the lining was most interesting. Some years ago Mr. Gourley had to inspect a number of miles of 32-inch diameter by $\frac{5}{16}$ - and $\frac{3}{8}$ -inch thick steel pipes lined with a $\frac{1}{4}$ -inch thickness of bitumen, after they had been sub-

* "Notes on the Flow of Water through Spiral and Buttweld Pipes." Paper read before N.S.W. members of The Inst. C.E., on 20th August, 1934.

jected to test pressures varying, according to location, from 170 to 350 feet. Apart from cupping and other evidence of plastic flow of the hand-placed bitumen between the spigot end of the lining and the end of the lining of the socket, it was observed that there were blisters at irregular intervals, but almost always at, or near, the soffit. In places where those blisters appeared in what was considered to be an incipient stage, there was evidence of slight star cracking, and it was concluded that the blisters were the result of percolation through those cracks and the relatively rapid emptying of the main for inspection of the joints. In view of the Author's observations and the confirmatory experiments, it appeared as if air absorption were a possible explanation.

Some years ago, Mr. Gourley found that the bitumen lining at the top of 36-inch-diameter steel pipes which had lain out in the open for some time, exposed to tropical sun, had sagged and had left the metal over a width of about 12 inches and a length of 2 feet from the spigot. That led him to ask the Author whether, before the tunnel was filled for test purposes, the lining had been sounded for "drummy" patches, the presence of which might have been a predisposing cause for the blistering?

* * The Author's reply will be published later.—SEC. INST. C.E.

Paper No. 5206.

"Further Note on Flood-Hydrographs." †

By BERTRAM DARELL RICHARDS, B.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Mr. J. R. Daymond referred to the previous Paper* in which the Author had idealized the conditions and had justifiably argued that the time rate of growth of the contributing area was dependent upon the amount of water on the ground, and from that found the mean velocity of flow (hence t) from the hydraulic conditions prevailing. There appeared to be an error in the result $d = kit$ for the depth d on the ground at time t .

† Journal Inst. C.E., vol. 11 (1938-39), p. 585 (April 1939).

* B. D. Richards, "Flood Hydrographs." Journal Inst. C.E., vol. 5 (1936-37), p. 405 (March 1937).

The equation would be true if the rain fell suddenly, in the form of a cloud-burst, but for the usual type of storm to which catchments were subjected,

$$(\text{Volume of rain}) - (\text{Volume soaked into ground}) = \\ (\text{Volume of run-off}) + (\text{Volume on area}),$$

or

$$akit = \int_0^t Qdt + da.$$

Hence

$$d = kit - \frac{1}{a} \int_0^t Qdt,$$

d denoting the average depth of water on the catchment area a after time t . The second term, being comparable in magnitude with the first in that expression for d , could not be neglected. That was important, because from it was derived the time of concentration t , given by equation (2) (p. 586 §), and also the shape of the hydrograph, so that any error arising on that point would prevail throughout the Paper.

Following the method suggested by the Author for the determination of t , a closer representation of the prevailing conditions was maintained than was possible by the more usual method of assuming a constant rate of flow over the area throughout the storm period. In the final result for t (equation (2), p. 586 §), however, it seemed that any advantage that the method possessed was immediately discounted by the inevitable approximation required in the estimation of s for a natural catchment. In the first place, s was meaningless when applied to an undulating area. Secondly, any value that might be ascribed to it had no more claim to accuracy than was warranted by estimating t directly without recourse to any theory. That criticism was not directed against the Author's method of approach, but merely pointed out what he (Mr. Daymond) had encountered when considering that aspect of the problem: any process of refinement in the analysis was always accompanied in the final result by a factor as complex as the one for which an equation had been obtained. Mr. Daymond fully appreciated the difficulty of recognizing and allowing for s , and had found it necessary in his own Paper, "The Estimation of Run-off from Areas Subjected to Rainstorms"[†], to assume a constant velocity over the area during the storm period, and then to accommodate variations in s by suitable modification of the time-area curve. Such assumptions had no greater merit than those proposed by the Author, but they led to results which were otherwise unattainable, and, what was more important, they provided the means of examining the effect on the

§ Page numbers so marked refer to the Paper. (Footnote (†), p. 504.)—SEC. INST. C.E.

† Paper No. 5123. *Abstract published in Journal Inst. C.E.*, vol. 6 (1936-37), p. 268 (June 1937). [The MS. and illustrations may be seen in The Institution Library.—SEC. INST. C.E.]

hydrograph due to all the variations from conditions of uniformity enumerated on p. 588 §, taken separately or together.

With regard to variation (2), p. 588 §, equation (1) (p. 585 §) contained the term $f(a)$ to account for the positional variation of i over a . That constituted a special feature of the Paper, but it seemed that the general results and conclusions were somewhat restricted by devoting most attention to conditions of uniformity of i and treating its variation as a special case. The inclusion of the term $f(a)$ at once postulated that the intensity of rainfall, as measured at different stations over the area, was not constant. It would, therefore, be of value to the results if that variation had been maintained for all results in the Paper. It might be of interest to take a definite example and to suggest a means of solution, taking into account eight variations of i . The results obtained could be utilized to discuss some further points of interest mentioned by the Author. Mr. Daymond presented an example showing how variations in assumptions (2), (3), (4) and (5) (p. 588 §) might be included, and from the fuller account ‡ it could be shown that the theory was capable of extension to include also variations of assumptions (1) and (6).

I_{xyt} was assumed to denote the intensity of rainfall at time t on an element of area $\delta x \delta y$, the time of flow from that element to C (Figs. 14 (a)) being $\tau - t$. Measured from the beginning of the storm ($t = 0$), the run-off δQ_τ from $\delta x \delta y$ due to I_{xyt} would thus reach C at time τ . If I_{xyt} were constant with respect to t (Figs. 14 (c)), then the run-off at time τ was

$$Q_\tau = \int_{a_1} K I_{xy} \delta x \delta y \dots \dots \dots (4)$$

If the duration of the storm $T = \lambda$, the time of concentration Q_τ was a maximum when a was a maximum ($a_1 = a$), and (4) then became :

$$Q_m = aki,$$

which agreed with equation (3) (p. 586 §) putting $a = 1,000$. It would be seen, also, that for I constant with respect to t , the run-off was a maximum when the whole area contributed at C, irrespective of the shape of the area and of the nature of the space variation of I . It was, therefore, not clear how the Author arrived at the conclusion (p. 594 §) that, "If the intensity of rainfall varies over the catchment area, the condition which will produce the maximum flood-intensity is that in which the heaviest rainfall occurs near the point of concentration." Mr. Daymond presumed that the point referred to corresponded to C, Figs. 14 (a).

If I_{xyt} were regarded as a function of t and of position, as indicated in Figs. 14 (b), consideration might be given to the different values of I_{xyt} , ($1I_{xy}$, $2I_{xy}$, $\dots \dots \dots$, nI_{xy}) at some particular time t falling on the

§ *Ibid.*

‡ Footnote (‡), p. 505.

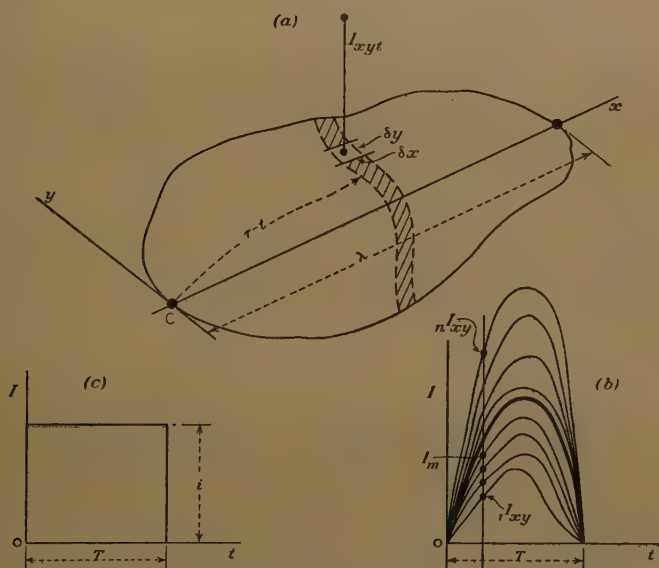
corresponding elements $(\delta x \delta y)_1, (\delta x \delta y)_2, \dots (\delta x \delta y)_n$. Considering a strip of those elements situated a distance from C (Figs. 14 (a)) corresponding to time $\tau - t$, the run-off at C from that strip at time τ would be given by :

$$\begin{aligned} \Delta Q_\tau &= [(k \delta x \delta y)_1] [I_{xy}] + \dots + [(k \delta x \delta y)_n] [I_{xy}] \\ &= (n k \delta x \delta y) \frac{1}{n} \sum_{\sigma=1}^n I_{xy} \end{aligned}$$

Assuming K to be the mean value of k over the strip, and proceeding to the limit,

$$\Delta Q_\tau = (K \Delta a)_{\tau-t} I_m \quad \dots \quad (5)$$

Figs. 14.



The first term of (5) indicated an impermeable strip $\tau - t$ from C, and I_m indicated the mean value of I_{xyt} at time t , shown by a thick curve in Figs. 14 (b). The analysis might be extended by assuming K to have a time variation $f(t)$. If $I_m = F(t)$ and $(K \Delta a)$ were represented on a time basis by $\phi(t)$, then substituting in (ii) :

$$Q_\tau = \int \phi(\tau - t) \cdot f(\tau - t) \cdot F(t) \cdot dt, \quad \dots \quad (6)$$

the limits of integration depending upon the relative values of τ , T , and λ .

Mr. Daymond presented an example in which $T = \lambda$; in such case it might be shown ‡ that the limits were :

$$0 \text{ to } \tau \text{ for } 0 \leq \tau \leq T, \text{ and } \tau - T \text{ to } T \text{ for } T \leq \tau \leq 2T.$$

‡ Footnote (‡), p. 505.

Further conditions assumed were $I_m = 6it(T - t)/T^2$, that was to say, a parabolic curve with i the mean of that curve; the area was assumed to be rectangular with K increasing uniformly from zero at 0 to a maximum at C (Figs. 15 (c)). K was assumed, also, to vary uniformly with respect to time (Figs. 15 (b)) so that

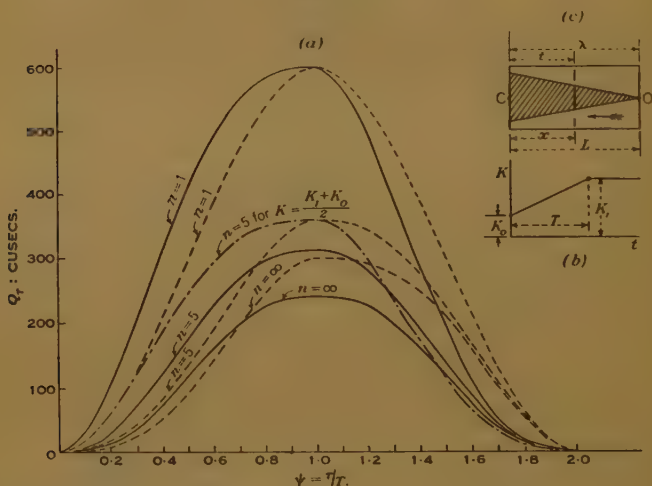
$$KT = (K_1 - K_0)t + K_0T \text{ for } 0 \leq t \leq T, \text{ and } K = K_1 \text{ for } t > T.$$

For conditions of uniform flow :

$$x/t = L/\lambda, \text{ and } \phi(t) = K(\Delta a) = 2A(T - t)/T^2,$$

A being the total area. Substituting in (6), putting $nK_0 = K_1$, $\tau/T = \psi$,

Fig. 15.



and integrating :

$$Q_\tau = \frac{K_1 A i \psi^2}{5n} \{30 + 20(n - 3)\psi - 15(2n - 3)\psi^2 + 12(n - 1)\psi^3\}$$

for $0 \leq \psi \leq 1$,

and :

$$Q_\tau = \frac{K_1 A i (2 - \psi)^3}{5n} \{20n + 15(1 - 2n)(2 - \psi) + 12(n - 1)(2 - \psi)^2\}$$

for $1 \leq \psi \leq 2$.

Taking $A = 1,000$ acres, $i = 1$ inch per hour, $K_1 = 0.6$, Mr. Daymond showed the Q_τ -curves (full lines) in Figs. 15 (a) for $n = 1$ ($K_0 = K_1$), $n = \infty$ ($K = 0$), and $n = 5$. The two extreme values of n , (1 and ∞), corresponded to the respective conditions of complete saturation and complete absorption at the beginning of the storm. It was interesting to note the marked difference both in the shape and peak values of the two hydrographs resulting from that time variation of K . Natural conditions on

a catchment-area would be more nearly approached by some intermediate value of n ; the curve for $n = 5$ was not intended to have any such significance.

The chain-dotted curve for $n = 5$ was obtained as before, except that K was considered to remain constant with respect to time at its mean value ($= (K_0 + K_1)/2 = (n + 1)K_1/2n$). It was interesting to compare that curve with the corresponding curve for $n = 5$ with K variable, and to note the difference in shape and peak value. In that respect, the Author stated (p. 599 §) that "the net effect of the variation of K would be to give results little different from those based on an average value." The foregoing indicated that that was not necessarily true; the matter was, however, of academic interest only, because the worst conditions were given by $n = 1$, both for volume of flow and maximum rate, conditions for which it would be necessary to design flood-control works.

In order to show the effect on the hydrograph from the variation of K over the catchment-area (p. 598 §) in comparison with K constant, the dotted curves, *Figs. 15 (a)* were drawn for K constant over the area, with the same total impermeable area as in the previous case. Again, comparing curves with corresponding values of n , the results did not confirm the Author's suggestion that "it seems unlikely that the results [for K variable over the catchment] would be very different from those given by taking an average value of K ." The closest agreement was obtained for $n = 1.0$, which figure was usually adopted for design. The calculations for those curves were not involved, and might be easily followed by reference to, and evident modifications of, the example already given in some detail.

It was gratifying to note the Author's recognition that nothing more should be expected from theory than a clearer understanding of the mechanism of the problem of the relationship between rainfall and run-off. Mr. Daymond stressed the danger of relying upon theory alone. He suggested, however, that the theory should be diligently pursued and that the results of the academic investigation be allied with comprehensive gauge records.

Mr. J. M. Lacey observed that the Author's hydrographs, as shown in *Fig. 3* (p. 589 §) and elsewhere, indicated that with a rising flood the rate of discharge increased with the magnitude of the flood discharge. If that were the case, there would be no great rise of flood water at the outlet, or gauging station, under consideration; the possibility of disastrous floods due to peak rises would, therefore, be remote.

In nature, a catchment-area was served by a main channel of a certain length, slope, and capacity, into which various minor streams flowed at different points, each tributary having its own basin. It was possible, in the earlier stages of the flood, that the rate of discharge would increase

with the magnitude of the discharge; but as the channels of the main stream and the tributaries filled up, the surface of the flood water would rise, and, although the magnitude of the flood was increasing, its rate of increase would be decreasing. If the capacity of the main stream were insufficient to carry the flood water, the level of the flood water would rise over the channel margins, and low-lying lands would be flooded, thus causing a further reduction in the rate of increase of the flood magnitude at the outlet. In a falling flood, provided no rain fell, the rate of flow would decrease as the flood subsided. The shape of the hydrograph curve, therefore, would be, for a rising flood, concave outwards only near the base, converging into a convex curve outwards as the magnitude of the flood increased. The falling-flood curve, provided no rain fell, was a fairly smooth curve, concave outwards.

In that connexion, attention might be drawn to the Royal Geological Society's final report on hydrograph profile †, and to the numerous hydrographs of rivers in the United States of America, published ‡‡.

Fig. 16 showed the hydrograph of the Penner river, South India, at the Sangam anicut during the flood of November 1903 *. The discharges were the calculated discharges flowing over the anicut. The maximum peak level was reached at 9 a.m. on the 12th November, when 8.2 feet was registered up-stream of the anicut. Unfortunately no note was made of the level of the water downstream of the anicut, showing the depth to which the crest was drowned. The figure for the discharge at 9 a.m. on the 12th was approximate.

Table IV gave the weighted mean rainfall over the basin of the river above the Sangam anicut, taking into consideration the areas of the various catchment-areas contributing to the flow and was extracted from the rainfall data given **.

TABLE IV.—MEAN RAINFALL IN INCHES OVER PENNER BASIN, 20,100 SQUARE MILES, NOVEMBER 1903.

Date.	Rainfall.	Date.	Rainfall.	Date.	Rainfall.
3	0.218	7	1.014	11	1.63
4	0.307	8	0.798	12	0.823
5	0.631	9	0.452	13	0.224
6	3.111	10	1.388	—	—

† J. S. Owens, "Report on Severn Discharge and Rainfall in the Basin." The Investigation of Rivers, Final Report, Royal Geographical Society, London, 1916, pp. 10 and 11.

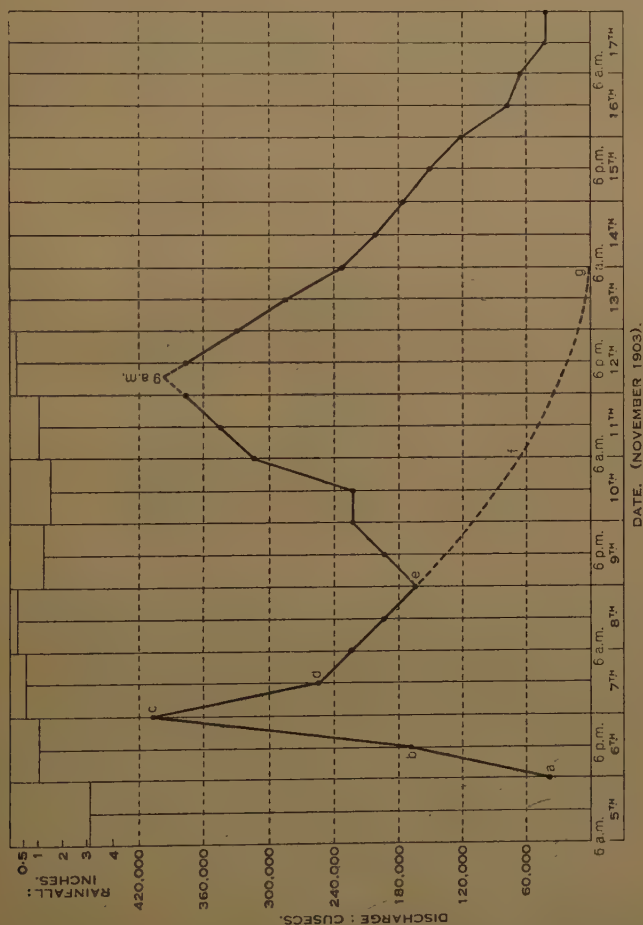
‡‡ Paper No. 772, "Studies of the Relation of Rainfall and Run-off in the United States." United States Department of the Interior, Water Supply.

* J. M. Lacey, "Floods in Southern India." Minutes of Proceedings Inst. C.E., vol. clxxi (1907-08, Part 1), p. 360.

** Pp. 369 and 370 of Paper referred to in footnote (*), above. There was a misprint on p. 370 of that Paper. The last two lines of figures should be transposed.—J. M. L.

The figures given in Table IV were those read at 8 a.m. every day, and showed the rainfall during the previous 24 hours. In *Fig. 16* the rainfall was shown on the day it had fallen. That was to say, the rainfall of 3.11 inches registered on the morning of the 6th was shown to have fallen

Fig. 16.



DATE. (NOVEMBER 1903).
HYDROGRAPH OF PENNER RIVER AT SANGAM ANICUT.

on the 5th; 1.63 inch registered on the morning of the 11th was shown to have fallen on the 10th, and so on. There was a lag of a day between the peak rainfall on the 5th and the peak flood at 6 a.m. on the 7th; similarly there was a lag of a day between the peak rainfall of 1.63 inch on the 10th and the peak flood at 9 a.m. on the 12th.

The "unit-hydrograph" analysis of surface run-off was fully described in the United States Geological Paper previously quoted. Mr. Lacey then attempted to apply that analysis to the flood of November, 1903, in

the Penner river. The Sangam anicut system* was a reservoir-filling system. For the greater part of the year there was little flow down the river, so that the ground or spring flow was negligible. In *Fig. 16* the hydrograph abcdefg (the flood water flowing over the anicut crest) was assumed to be that due to the peak rainfall of 3.111 inches on the 5th November, since the peak floods, causing the water to surplus over the anicut, followed that peak rainfall. The 8,000 cusecs passing through the under sluice, and into the main canal of the Sangam anicut system, was assumed to be due to the rainfall previous to the 5th. Table V showed

TABLE V.

Date.	Run-off from unit station : cusecs.	Distribution graph.
5	0	0
6	36,000	0.034
7	397,753	0.385
8	220,865	0.213
9	161,426	0.156
10	108,000	0.104
11	66,000	0.063
12	36,000	0.034
13	12,000	0.011
Total . .	1,032,046	1.000

the run-off from the limit rainfall of 3.111 inches, taken from the 6 a.m. discharges of the curve abcdefg, and the computed distribution graph.

Table VI showed the development of the pluviograph (*Fig. 17*), or

TABLE VI.—PENNER RIVER BASIN. CATCHMENT AREA : 20,100 SQUARE MILES.

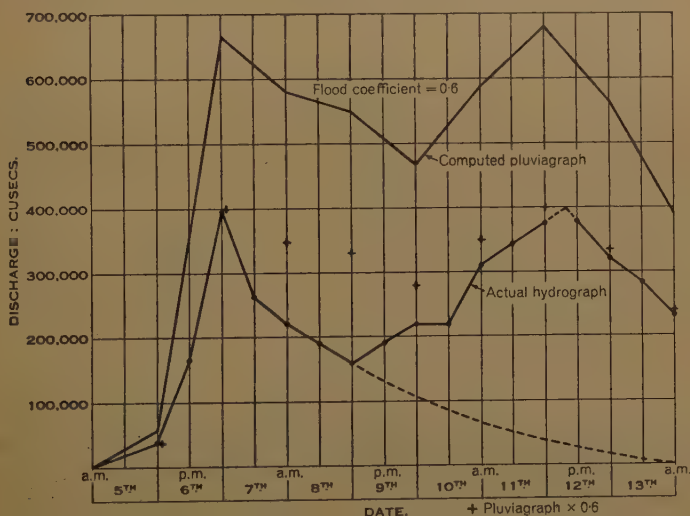
Date.	Rain-fall.	Dis-tribution.										Total inches in 24 hours.	Pluviograph or 100 per cent. run-off: cusecs.
Nov. 1903.													
5	3.111	0	0										
6	1.014	0.034	0.106	0								0.106	56,985
7	0.798	0.385	1.198	0.035	0							1.233	662,800
8	0.452	0.213	0.663	0.390	0.027	0						1.080	580,600
9	1.388	0.156	0.485	0.216	0.307	0.015	0					1.023	550,000
10	1.630	0.104	0.323	0.158	0.170	0.174	0.047	0				0.872	468,787
11	0.823	0.063	0.196	0.106	0.125	0.096	0.534	0.055	0			1.112	597,811
12	0.224	0.034	0.106	0.064	0.083	0.071	0.296	0.628	0.028	0		1.276	685,977
13	—	0.011	0.034	0.034	0.050	0.047	0.217	0.347	0.317	0.008		1.054	566,000
14				0.011	0.027	0.029	0.145	0.254	0.175	0.086		0.727	390,835

* Footnote (*), p. 510.

100 per cent. run-off, from the distribution graph, applied to each day's rainfall in succession. *Fig. 17* shows the pluviograph compared with the actual hydrograph.

The "flood coefficient" was the ratio between the greatest ordinate of the hydrograph of surface run-off and that of the pluviograph. "This ratio is not an average coefficient for the flood period, it insures agreement between the observed and computed peak values, with only a slight sacrifice in agreement between the actual, and computed hydrographs" ††. The coefficient for any particular basin would vary with the season of the year.

Fig. 17.



The available records of rainfall showed only the total rainfall during the previous 24 hours, and did not indicate the difference in intensity and duration of the falls. It would be desirable to have a continuous record of rainfall showing the beginning and end of all storms, and their intensities.

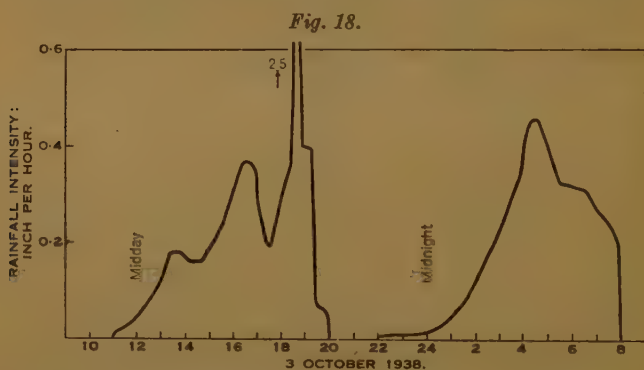
Mr. David Lloyd realized the difficulties in assuming values of the coefficients required, and, in particular, in making the assumption of uniformity. He presented some data obtained at lake Vyrnwy, which supplied water to the city of Liverpool.

Vyrnwy drainage basin had an area of 36.4 square miles, an average annual rainfall (1881-1915) of 69.7 inches, and an average temperature of 43.8° F.; the average elevation was 1,453 feet; the distance from the point of interception to the farthest point was 7.9 miles; the average

†† Footnote (††), p. 510.

slope was approximately 1 in 34. The surface cover was of Lower Silurian shales, whilst the shape was approximately square. At that area, the water day commenced at 8 a.m.; rainfall was computed by the cartographical method, and the intensity of rainfall was measured by Dines recording rain-gauges.

On the 3rd October, 1938, heavy rainfall was experienced over the Vyrnwy catchment-area, the average over the area being 4.13 inches in the day, a value only previously surpassed once in the previous 42 years. Rain had fallen on the 6 preceding days after a dry summer, yet the daily run-off was only 40 per cent., the remainder being discharged up to 76 per cent. with flow to the end of the month. With a general fall of 2.24 inches in 9 hours, a peak flood of only 188 cusecs per 1,000 acres was produced. The



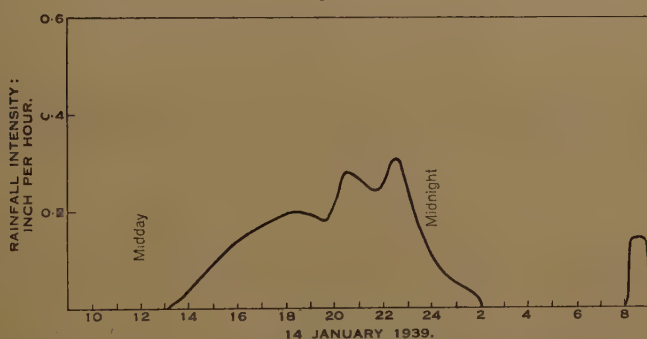
rainfall, as indicated in *Fig. 18*, was in two periods from 11. a.m. on the 3rd to 8 p.m. on the 3rd, and from 10 p.m., on the 3rd to 8 a.m. on the 4th. The greatest intensity of rainfall was 2.5 inches per hour at 6.47 p.m. on the 3rd, though the average intensity over the 9 hours was 0.25 inch per hour. The peak flood was measured at 7.30 p.m. (3rd), just within the first rain period, or $8\frac{1}{2}$ hours after rain commenced. The temperature was 49° F. The storm followed a dry period, hence the requirements of soil-moisture were bound to have considerably reduced the surface flow.

By way of contrast, the storm of the 14th January, 1939, gave 2.12 inches during the rainfall day, producing a run-off of 64 per cent. The value was higher than usual, possibly because 2 inches of snow (equivalent to 0.2 inch of rain) had been lying on the catchment, which, if added to the day's rainfall, would reduce the run-off to 58 per cent. At the end of that month 93 per cent. of the month's rain had been discharged. The average intensity during the rain period of 13 hours from 1 p.m. (14th) to 2 a.m. (15th) was 0.132 inch per hour, the maximum intensity being 0.31 inch per hour; the peak run-off, however, reached 223 cusecs per 1,000 acres at 4 a.m. on the 15th (two hours after rain ceased or 15 hours after rain commenced). Incidentally, the temperature had dropped below zero

during the night and that might have been expected to reduce the run-off. Further intense rain fell shortly before the 24 hours' end, completing the stated total, but the falling-flood curve was only slightly affected. The variation of i in time for that flood was shown in *Fig. 19*.

Since the Vyrnwy area was almost impermeable, high peak values of surface flow might be expected. The peak discharges described had been exceeded previously, as on the 3rd November, 1931, when the average intensity of rainfall was 0.177 inch per hour, which produced a maximum peak of 365 cusecs per 1,000 acres from a rainfall closely resembling, in total, that of the 3rd October, 1938. The difference in peak-discharge values was striking. The peak discharge occurred in that flood 13 hours

Fig. 19.



after rain commenced. Assumption of a wet catchment-surface doubtless played for safety; the conditions preceding the flood of the 3rd October, 1938, considerably reduced the peak. The foregoing data suggested that the coefficient K might be considerably higher than the values discussed in the Paper.

The Author, in reply, thought that the Correspondence contained some valuable constructive criticism. To claim too much for a formula of that nature was the surest way to discredit it, but he claimed rather more than was implied by Mr. Daymond's concluding remarks. In course of time a mass of data would, no doubt, be accumulated in most developed countries, which would enable flood conditions to be assessed without recourse to formulas. Where such data were lacking, formulas became necessary. It was essential that they should be simple in application, that they should be reasonably true to theory, and that they should provide for the principal factors affecting floods. In view of the complexity of the factors, a formula covering all conditions and theoretically correct would be too cumbersome to handle, even if it could be deduced. He thought that the formula proposed met the conditions set out above, and that it might be used with some confidence for the estimation of the worst probable conditions of flood, where those conditions could not be deduced

from actual records. He might point out, however, that it was devised for the estimation of worst probable flood conditions, rather than for the prediction of floods under varying conditions of rainfall. It also presented the advantage of enabling the flood hydrograph to be determined, which was essential in problems of flood regulation.

In the application of the formula the difficulty lay, obviously, in the correct assessment of the factors, but in the Author's view that was a real difficulty only with respect to the coefficient K , with which he would deal later. Mr. Daymond had suggested that the coefficient s was difficult to assess and that in undulating catchments it became meaningless. There was generally, however, a main river extending for the greater part of the length of the catchment area and the Author thought that s might be taken as the mean gradient of that river. It was not clear to him how t , the period of concentration, could be determined except from theoretical considerations, unless such complete flood data existed as might obviate the use of a formula.

Mr. Daymond had expressed the run-off of a catchment area in the equation :

$$akit = \int_0^t Q \cdot dt + d \cdot a,$$

where

$$d = kit - \frac{1}{a} \int_0^t Q \cdot dt,$$

where d denoted the average depth of water over the catchment at t , the period of concentration. He thought that the Author had, in the derivation of the formula in his original Paper, ignored the second term as negligible. It was very far from being negligible nor had it been treated as such. The depth of water on the catchment at 0 increased from 0 to Kit in the time t , the duration of flood and period of concentration. At A , the head of the catchment, it remained at 0. The velocity of run-off increased from 0 to $c\sqrt{Kits}$ in time t , its average being $\frac{2}{3}c\sqrt{Kits}$. That point had been a little obscure in the Author's original Paper, but had been cleared up in the reply to the Correspondence †. During the period of concentration, less than half of the total yield ran off the catchment, and the term da was appreciably greater than the term $\int_0^t Q \cdot dt$. At t , the flood reached its peak; no more rain fell and the further run-off was derived from the water accumulated on the catchment, da . During the period of falling flood, the run-off was expressed by the equation :

$$\int_1^{2t} Q \cdot dt = da.$$

† Correspondence on "Flood-Hydrographs," Journal Inst. C.E., vol. 6 (1936-37), p. 471 (October 1937).

With respect to the coefficient $f(a)$, Mr. Daymond had taken that as implying more than had been intended; $f(a)$ was a corrective factor for area based on the established hypothesis that the average intensity of a rainstorm was an inverse function of its area. It did not refer to positional variation of i .

Mr. Daymond thought that the conclusion on p. 594 § was not clear. It read: "If the intensity of the rainfall varies over the catchment area, the condition which will produce the maximum flood-intensity is that in which the heaviest rainfall occurs near the point of concentration." The Author argued, however, that the average intensity of rainfall was an inverse function of the area and duration of the storm, and, further, that a very high intensity on part of the catchment area would imply a low intensity elsewhere. Taking the extreme case of the whole rainfall being concentrated on $1/n$ th part of the catchment, that part would receive ni , the remainder nothing. The nearer that area was to the point of concentration, the less was the distance that the water had to travel and the less would be the soakage and evaporation losses; hence the greater would be the flood discharge. It followed, then, that the more the rainfall was concentrated at the lower end of the catchment, the greater would be the flood-intensity per unit area of catchment.

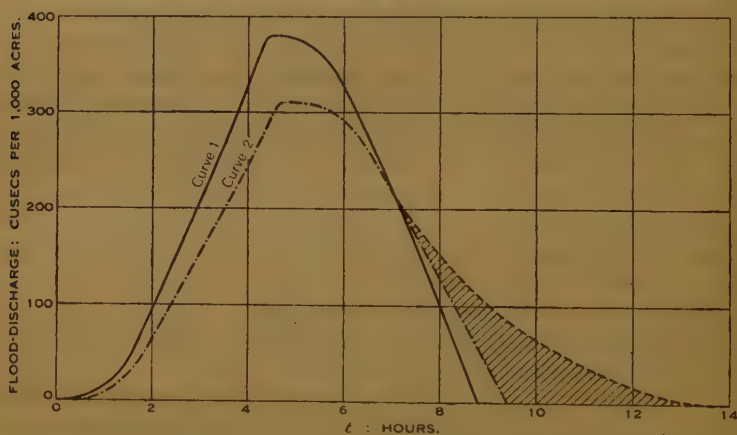
The coefficient K was, in the Author's view, the most difficult to assess. On p. 588 § of the Paper, he stated:

"The coefficient K represents the coefficient of immediate run-off. Of the water lost, $(1 - K)$, a part may emerge later as ground-water, and if this occurs within the period of the flood, it will give rise to an indeterminate tail to the hydrograph as suggested by the dotted line on curve 1 in *Fig. 2*."

Assuming the coefficient of ultimate run-off to be K_u , then $1 - K_u$ would represent the water totally lost, and $K_u - K$ the water temporarily lost but subsequently recovered as ground-water. The less porous the catchment, the nearer would K approach to K_u , and at the same time the higher would be the value of the latter. At the limiting condition of a totally impermeable catchment, $K = K_u = 1$, and there would be no after-flow or tail. If curves 1 and 2 (*Fig. 20*, p. 518) represented the hydrographs for $K = K_u$ and K , the coefficients of immediate and ultimate run-off respectively, then curve 2 would have an after-flow or tail represented by the hatched area, and the sum of the areas of curve 2 and that hatched area would equal the area of curve 1. The point at which the tail commenced would be the time at which ground-water began to emerge, and, generally speaking, the more porous the catchment the later that would be. At the same time the more porous the catchment the greater would be the difference between K_u and K , and the larger would be the hatched area. Reduction of porosity would produce the

converse effect, and at the limit of an entirely impermeable catchment, the tail would disappear and curves 1 and 2 would coincide. It would be seen that ground-water might materially affect the shape of the hydrograph. It was important to note that K was the coefficient of immediate run-off and not that of ultimate run-off. All catchments were, to some degree, porous, so that there would generally be a tail to the falling-flood curve which would tend to produce the concave shape referred to by Mr. Lacey.

Fig. 20.



The theory implied the absence of any restriction which would cause inundation. Inundation, as referred to by Mr. Lacey, introduced an entirely new factor. It caused the temporary storage of part of the flood-water and had the effect of damping down the flood and modifying the hydrograph. That was a local effect, which it might, in some cases, be possible to calculate. The Author used the word "flood" in the sense of "flood discharge" and not "inundation."

Mr. Lloyd had presented some valuable flood records for lake Vyrnwy catchment area. From the figures given, the Author thought that 0.6 was a reasonable figure to take for K , the coefficient of immediate run-off. That was, incidentally, the figure taken in his curves for British upland catchments in his former Paper*. He had analysed Mr. Lloyd's records in the following manner:—

- (1) Taking $R = 4$, the coefficient of rainfall likely to produce normal floods in Great Britain, and calculating from the formula, the theoretical flood, with a , $f(a)$, C , L and s determined from the data, the result was $t = 4.35$ hours, $Q = 337$ cusecs per 1,000 acres.

* "Flood-Hydrographs." Journal Inst. C.E., vol. 5 (1936-37), p. 405 (March 1937).

In abnormal storms R might have a higher value than 4 * which would increase the flood intensity.

- (2) The storm of the 3rd October, 1938, might be treated as two consecutive storms :

In the former, 2.24 inches of rain fell in 9 hours, the time-distribution of the rainfall approximating to case 10 of Table 1 (p. 597 §). The duration of the storm and the period of concentration were substantially the same. From the given values of i and T , R was deduced to be 3.32, and applying that to the formula, $t = 4.16$ and $Q = 242$ cusecs. The corrective factor for case 10 was approximately 0.80. That increased t somewhat whilst reducing Q to 194 cusecs per 1,000 acres. The recorded peak flood was 188 cusecs.

In the storm which followed after an interval of about 5 hours, 1.89 inch fell in 7 hours. The time-distribution curve approximated to case 11, giving a corrective factor of about 0.90. Proceeding in the same way, $t = 5.8$ and $Q = 191$. Applying the corrective factor, Q became 172 cusecs per 1,000 acres. That figure might be somewhat higher by reason of lag flow from the preceding flood, but the time interval would have enabled the latter to have subsided. Mr. Lloyd had not stated the recorded value of the second peak but had noted that the main peak followed the first flood.

- (3) The storm of 14th January, 1939, gave 2.32 inches of rain (including the accumulated snow) in 13 hours. The time-distribution curve approximated to case 13, which gave a corrective factor of about 0.90. The duration of storm and time of concentration were substantially the same. That gave $R = 3.33$, $t = 5.15$, and $Q = 244$. Applying the corrective factor, Q became 220 cusecs per 1,000 acres. The recorded value was 223 cusecs.
- (4) The storm of the 3rd October, 1931, gave a recorded peak flood of 365 cusecs for an average rainfall intensity of only 0.177 inch. The data were inadequate for an analysis but might possibly be accounted for by variation in the distribution of the rainfall in regard to time and position.

The hydrographs of the actual floods had not been given, but the above analyses appeared to show that the recorded peak floods were consistent with the theory for a value of K , the coefficient of immediate run-off, of 0.6.

In regard to the variations of K in respect to time and position, Mr. Daymond's mathematical presentation and his comparative curves

* See p. 407 of earlier Paper (Footnote (*), p. 518).

§ *Ibid.*

deduced therefrom, were of great interest. In the Author's view, however, the limits of variation of K for a given catchment would not be very wide, and in view of the inherent difficulty of assessing K , the refinement of introducing variations with regard to time and position would not be likely to lead to such increase of accuracy in the determination of Q as to justify the complication of the calculations.

CORRESPONDENCE
ON PAPERS PUBLISHED IN
JUNE 1939 JOURNAL.

Papers Nos. 5100 and 5194.

“Investigation of the Outer Approach-Channels to the Port of
Rangoon by means of a Tidal Model.”*

By OSCAR ELSDEN, M.Sc., Assoc. M. Inst. C.E.

and

“Schemes of Improvement for the Cheshire Dee: An
Investigation by Means of Model-Experiments.”†

By JACK ALLEN, M.Sc., Assoc. M. Inst. C.E.

Correspondence.

Mr. Gerald Lacey, of Lucknow, noted the advance which had been made in technique since the model-experiments of Professor Osborne Reynolds, and the corresponding increase in the quantitative value of results. In respect of the basic theory of models little advance appeared to have been made, and he suggested that both Authors might have been more explicit on the subject of model-scales, and the reasons which led them to adopt the precise exaggeration eventually employed.

Mr. Elsdén referred to two empirical formulas of Professor Reynolds which had been employed, whereas Mr. Allen referred only to the one which permitted the calculation of the tidal period of the model from the arbitrarily assigned horizontal and vertical scales. It appeared, however, that both Authors had employed only the basic equation which followed from the theory of wave motion. That equation was empirical in the sense that Professor Reynolds, by actual hand working of his first model, had very soon discovered that any tidal period, other than that computed, led to tidal phenomena such as bores or standing waves which were non-existent in the prototype.

It appeared that the horizontal scale was first assigned with reference to the available space in the laboratory, whilst an exaggerated vertical scale, which experience had shown merely to be of the correct order, was

* Journal Inst. C.E., vol. 12 (1938-39), p. 3 (June 1939).

† *Ibid.*, p. 30.

adopted; on the basis of those two scales the tidal period of the model, or, effectively, the time-ratio, was computed. From the horizontal and vertical scales assigned, and the time-ratio, t/T , all other relationships were derived. It was evident that exaggeration in the vertical scale of models was a function of scale, very large models being associated with small exaggerations, and very small models with large exaggerations. So far as Mr. Lacey was aware, little attempt had been made to evaluate that function for models, although Professor Reynolds had expressed a hope* that further investigations would lead to the determination of the "best proportional depths." In 1930, Mr. Lacey had attempted a solution† based on a study of channels transporting fine sand. The formulas took the very simple form:

$$\text{horizontal scale} = \left(\frac{t}{T}\right)^{\frac{2}{3}},$$

$$\text{vertical scale} = \frac{t}{T},$$

$$\text{and} \quad \text{exaggeration} = \left(\frac{T}{t}\right)^{\frac{1}{3}},$$

and were applicable to all river models in alluvium, the ratio t/T having reference to homologous times. The equations were derived from the basic velocity relation $V \propto R^{\frac{1}{2}}$, common to both wave propagation and scour, and from Mr. Lacey's basic equation $P \propto Q^{\frac{1}{2}}$, connecting the wetted perimeter and the discharge. From those relations it was clear that, once the horizontal scale had been assigned with reference to the accommodation available, all the other elements of the model were uniquely determined. Mr. Lacey suggested that of the elaborate technique involved in model tidal experiments, part was inevitable and part might be avoided if more attention were paid to precise exaggeration. It was certainly desirable to determine a "standard exaggeration" and to depart from that only when forced to do so, the "departure-ratio" being stated.

Adopting the horizontal scale of the Rangoon model as basic, Table I gave the actual scales and standard scales.

The scales for the Rangoon model as adopted, and as proposed by Mr. Lacey, both satisfied the equation in respect of wave motion and of scour. That was to say, both series of scales were in accordance with the principles set forth by Professor Osborne Reynolds.

The standard scales provided for less exaggeration and the departure-ratio in exaggeration for the Rangoon model was no less than 2.1. That departure made it easier to read observations but produced a curious effect in the derived scales.

* III. Internationaler Binnenschiffahrts-Congress zu Frankfurt am Main, 1888, vol. III, p. 335 (Proceedings are in French).

† Minutes of Proceedings Inst. C.E., vol. 229 (1929-30, Part 1), pp. 380, 381.

TABLE I.—RANGOON TIDAL MODEL.

Scales.	Adopted.	Standard.
Horizontal :	1 : 8,060	1 : 8,060
Vertical :	1 : 192	1 : 402
Velocity (horizontal) :	1 : 13.85	1 : 20.05
Velocity (vertical) :	3.04 : 1	1 : 1
Volume :	1 : $12,500 \times 10^6$	1 : $26,100 \times 10^6$
Discharge :	1 : 21.5×10^6	1 : 65.0×10^6
Exaggeration :	42.00	20.05

Mr. Lacey thought that Mr. Elsdén's notes on the subject of coagulants had a fundamental bearing on the precise exaggeration of the vertical scale, and the vertical velocity ratio. Mr. Elsdén had referred (p. 14 §) to the use by Professor Gibson of a coagulant solution in the river Severn model. The need for the use of a coagulant in the Severn model, and in the Rangoon model, would appear to be largely due to the departure from standard exaggeration. That departure led to a very remarkable anomaly in the vertical-velocity ratio. The velocity of deposition of fine silts in suspension was a constant, and if the same silt were to be used in the model as in the prototype the scales should be such as to make the vertical-velocity ratio unity. It would be observed that Mr. Lacey's standard scales assigned that ratio, whereas, if the actual Rangoon model scales were employed, the fine silt in the model had to be forced, by means of a coagulant, to deposit at three times the rate deposited in nature. That complication in technique was occasioned largely, if not solely, by departure from standard exaggeration.

Mr. Lacey would be glad to learn whether Mr. Elsdén had any objection to the standard scales which he had proposed, and whether, had they been employed instead of those adopted, a coagulant would have been used. It was possible that a coagulant might always be required, but it appeared essential to construct the model to standard scales so that a vertical-velocity ratio of unity was maintained. It was clear that the equations derived independently by Mr. Lacey were entirely consistent with a scouring and silting velocity, for the coarse bed material, varying as the square root of the depth, and a constant velocity of deposition of the fine suspended silt.

It was of interest to calculate standard scales for the two Mersey tidal models of Professor Reynolds, and in Table II (p. 524) actual and standard scales were compared.

It would be observed that the departure from standard exaggeration was relatively small in the larger model, and that in the smaller model the agreement was extraordinarily close. The Rangoon model was to a larger scale than the larger of the Mersey models and should, therefore,

§ Page numbers so marked refer to the Papers (Footnotes (*) and †), p. 521).—
SEC. INST. C.E.

TABLE II.—MERSEY TIDAL MODELS.

Scales.	Adopted.	Standard.
Horizontal :	1 : 10,600	1 : 10,600
Vertical :	1 : 396	1 : 482.5
Velocity (horizontal) :	1 : 19.90	1 : 21.97
Velocity (vertical) :	1.35 : 1	1 : 1
Exaggeration :	26.8	22.0
Horizontal :	1 : 31,800	1 : 31,800
Vertical :	1 : 960	1 : 1,004
Velocity (horizontal) :	1 : 31.0	1 : 31.7
Velocity (vertical) :	1.07 : 1	1 : 1
Exaggeration :	33.1	31.7

have been assigned a lesser exaggeration. It was, however, given an exaggeration of no less than 42.0, an exaggeration greater than that assigned by Professor Reynolds to the smaller of his models.

Referring to Mr. Allen's Paper, Mr. Lacey observed that the larger of the Cheshire Dee tidal models had an exaggeration almost identical with that of the larger Mersey model, but the scale, however, had been doubled. The smaller model departed greatly from standard exaggeration. Table III compared the actual scales with the standard scales.

TABLE III.—CHESHIRE DEE TIDAL MODELS.

Scales.	Adopted.	Standard.
Horizontal :	1 : 5,000	1 : 5,000
Vertical :	1 : 200	1 : 293
Velocity (horizontal) :	1 : 14.14	1 : 17.1
Velocity (vertical) :	1.77 : 1	1 : 1
Exaggeration :	25.0	17.1
Horizontal :	1 : 40,000	1 : 40,000
Vertical :	1 : 400	1 : 1,170
Velocity (horizontal) :	1 : 20	1 : 34.2
Velocity (vertical) :	5.0 : 1	1 : 1
Exaggeration :	100.0	34.2

As the water of the Dee was said to be clear the departure of the vertical-velocity ratio from the standard was not so important in the Cheshire Dee model as in that of the Rangoon model. The standard scales put forward had reference to all river-model work, but in tidal models, as Mr. Allen had remarked in his reply to the Discussion, the alternating effect of flood and ebb greatly assisted in making the rate of change of the bed approximately the same for different materials within limits. In tidal models the velocity was, to a great extent, enforced, whilst in ordinary river models it was free, and for such models departures from standard exaggerations would affect not only the rate of deposition

but also the rate of scour. It could, of course, be argued that the use of coagulants was enforced by the use of abnormal exaggerations in the vertical scale, and until models were constructed to standard scales it was impossible to interpret the conclusion of Professor Gibson that the size of sand used should be about three-quarters the size of the actual sand in the river bed. The change in the size might arise in part from departures from standard scales.

It appeared, from a study of the results obtained from modern tidal experiments, that the observations of tidal levels in the smaller channels and creeks were less reliable than those in the estuary. That arose from the fact that, in the narrower channels, the ratio of the wetted perimeter to the water width was greater. That was unavoidable, but it was very evident that any abnormal exaggeration of the vertical scale was to be avoided, as it increased the bottle-neck effect and caused the model to record higher levels than existed in the prototype.

In making his observations, Mr. Lacey did not intend to detract in any way from the great practical value of the experiments carried out on models of the Rangoon estuary and of the Dee, but he hoped that some concerted attempt would be made to determine the basic theory of river-model work. Whether the standard scales he had suggested subsequently proved to be correct or not, it was clear that all model-scales should be referred to some standard, and he could not think of a better standard than one which incorporated the methods of Professor Reynolds, and which, in addition, secured a uniform vertical-velocity ratio. For ordinary suspended silt that ratio should be unity.

Mr. James Mitchell observed, with regard to Mr. Elsdon's Paper, that reference was made on p. 20 § to "the ripple-formation inevitable in all moveable-bed models", and on p. 24 § it was stated that, as the result of surveys made by the Port Commissioners in 1932, the bed-material in the model was changed from sand to silt. Assuming the mean diameter of the grains of sand used in the model to be 0.007 inch, and neglecting the horizontal scale, they would correspond with pebbles having a mean diameter of about $1\frac{1}{3}$ inches, and it was highly improbable that tidal currents existed on the actual sea-bed sufficient to move such material. Although it was probable that the actual tidal-wave should be regarded, in general, as being a carrier of previously-loaded material, rather than as being itself an erosive agent, the much greater relative speed of the model-waves was likely to cause an erosive effect which was absent in the actual sea-bed. That conclusion was supported by the ripple-formation referred to.

It was stated, on p. 15 §, that the fans used for producing the model-waves were adjusted so that the waves had the correct height from trough to crest. The object aimed at, however, was to reproduce the effect of

the waves upon the bed-material, and the mere height of waves was no proper measure of that. There was a wide difference, even although they were of the same height, between the "surface-wave" produced during the early stage of a severe gale, and the "ground-swell wave" which existed at the end of the gale. The latter agitated the sea to a greater depth, and exerted an enormously greater destructive force. It was difficult to make the fans deal, adequately, with the tendency of winds to blow in gusts of varying intensity and period, or with their habit of changing in direction during the course of a gale and thus producing changes in the character and intensity of the waves. They failed also in dealing with the "fetch" of the wind, which exercised a profound and little-understood influence on wave-formation. It was probable that, with the small size of the model-waves, the effects of viscosity and surface tension would be disproportionately greater than in large-scale waves.

On p. 18 § it was stated that model gauge-readings were taken at intervals corresponding to 45 minutes on the natural scale, and that "the machinery was adjusted until the model tidal wave bore a reasonable resemblance to the real one." A 45-minute interval was a long one for such a purpose, especially in the regions of mid-flow and mid-ebb during spring tides. Moreover, although the two sets of tides resembled each other, it did not follow that the ratio between the forces producing them was correctly measured by that founded on the time and linear scales of the model.

It would be interesting to know why the dredged channels were formed by simply stirring up the bed-material, and allowing the tidal current to carry it away, instead of by the more orthodox method of dredging. If the material had been sucked up into a containing vessel, it would have given some indication of the probable quantity necessary to be dredged, so as to produce the required channel. Experiments could also have been made as to the comparative merits of dredging inward from the seaward end of the channel, or outward from its river end.

The curve in *Fig. 1* (p. 56 §), submitted by Mr. Guthrie Brown, was interesting. It showed a very satisfactory condition of affairs on the outer bar of the river, and it was to be hoped that the increase of depth would continue. A consideration of the portion of the curve relating to the period during which the model was in operation did not, however, give much support to the idea that the portion from about March, 1936 onward could have been reasonably inferred from it.

A consideration of the arrangements for dealing with the various difficulties involved led to the conclusion that the results of model-experiments in that field ought to be accepted with a great deal of caution, and ought to be regarded as valuable rather from a qualitative than from a quantitative point of view. That was particularly the case if the experi-

ments were extended so as to deal with a considerable sequence of years. In view of the erratic character of the effects produced by floods and storms, it might indeed be said that if a model, starting from the conditions found by actual survey of a portion of an estuary or of the open sea, and operated over a considerable number of years, showed results which were confirmed by actual survey at the end of the period, that could only be regarded as a remarkable and highly suspicious coincidence. Nevertheless, if the results of model-experiments were properly used they had a very great value, and the method was the best that had yet been devised for the investigation of many difficult sea and river problems.

Mr. Elsden, in reply, observed that the principle evolved experimentally by Osborne Reynolds was that, in order to ensure the correct turbulence of flow in any model, h^3e was bound to be not less than 0.09, where h denoted the tidal range at the seaward end of the model, and e the vertical exaggeration of scale referred to a 30-foot tide. That principle was adopted in choosing the vertical scale for the Rangoon model, the horizontal scale having been determined by site-conditions. As constructed, the value of h^3e was $\frac{30^3}{192^3} \times 42$, or 0.161. With the "standard"

scales, as proposed by Mr. Lacey, the value of h^3e would be $\frac{30^3}{402^3} \times 20.5$, or 0.0086, a value well below the limits given by Reynolds.

The Severn and Humber models, constructed prior to the design of the Rangoon model, had been based on the Reynolds criterion, and had given good results. As the Rangoon model was intended to answer specific questions within the shortest possible time, it had been considered advisable in that instance to adopt a principle which had already been well tried in more or less similar conditions.

So far as Mr. Elsden was aware, no tidal model yet constructed in Great Britain had had its vertical scales chosen according to Mr. Lacey's suggestions. Mr. Lacey's "standard scales" appeared to give, in every case, a smaller exaggeration than would be the case were Reynolds' criterion adopted. That was not, however, an argument against the use of Mr. Lacey's standards, for one of Reynolds' Mersey models (the one mentioned in the lower part of Table II) (p. 524, *ante*), whose scales were well outside the limits proposed by Reynolds, was stated to have given very close results.

Mr. Elsden had to state that he saw no basic objection to the use of Mr. Lacey's standards, even though they involved some departure from what might now be regarded as accepted practice. In the particular case of the Rangoon model, however, the use of Mr. Lacey's standards would make the model tidal ranges much too small; the spring tide, for instance, would be reduced to $\frac{1}{2}$ inch, and the neap tide would only be $\frac{1}{4}$ inch. With ranges as small as those, the distorting effects due to surface tension would, in Mr. Elsden's view, begin to be undesirably great.

A coagulent solution would have been used for the model sea-water, irrespective of the vertical scale adopted. It was considered that the sudden mixture of a fresh-water stream with a large salt-water body was an important factor in the formation of silt deposits near river mouths.

With regard to the exaggeration of 42.0, he would point out that that figure compared well, for instance, with 42.5 and 85 for the Severn model and 37.5 for the present Mersey model.

Regarding Mr. Mitchell's query, it was not considered that the effect of wind-waves was a major factor in determining the regime of the estuary, and it would, of course, be impossible to reproduce accurately all the vagaries of the prevailing winds or of exceptional gales. It was definitely impossible to produce model wind-waves which were accurate replicas in all respects, and the method adopted in the Rangoon model was considered to be the most suitable compromise.

The interval of 45 minutes for the tidal observations was only 5 seconds when considered from the model point of view. It was difficult to reduce that very much further, in view of the impracticability of making any reliable form of autographic recorder.

The method of dredging by "agitation" was adopted, as it was considered that that method might have great advantages in actual practice. It was considered that dredging from the river end of the channel would give the best results.

Mr. Allen, in reply, appreciated Mr. Lacey's remarks regarding the desirable relationship between the horizontal and vertical scales of a tidal model. He did not feel, however, that the evidence so far available was a direct confirmation of Mr. Lacey's theory, although it might be agreed that the exaggeration of the vertical scale relative to the horizontal scale would be some function of the horizontal scale. Mr. Lacey suggested that for a given horizontal scale of $1 : x$, the exaggeration should be $x^{1/3} : 1$, and certainly two Mersey tidal models of Professor Osborne Reynolds agreed extremely closely with that theory. At the same time, however, it ought to be borne in mind that Reynolds himself stated * : "From my present experience in constructing another model, I should adopt a somewhat greater exaggeration of the vertical scale."

Reference might also be made appropriately to Professor Gibson's Severn models, which had a horizontal scale of $1 : 8,500$. In the first case a vertical scale of $1 : 100$ was adopted, but the major part of the work was repeated (and additional tests made) with a scale of $1 : 200$. Thus the vertical exaggerations were $85 : 1$ and $42.5 : 1$ respectively, compared with $20.4 : 1$ as required by Mr. Lacey's formula. It was important to observe that the conclusions reached from the two models regarding the probable effect of a tidal barrage were in close agreement, two very

* Osborne Reynolds, "Papers on Mechanical and Physical Subjects," vol. ii (1881-1900), p. 335. Cambridge, 1901.

striking instances being detailed in Professor Gibson's Reports *. From that it would appear that in some tidal-model investigations, a definite advantage might be gained by starting with a vertical exaggeration rather on the large side. Due to the relatively short tidal period accompanying a large vertical exaggeration, results might be quickly obtained of the effect of different proposed schemes of improvement. Those results would possibly not be conclusive, but they would, in general, be sufficiently qualitative to warrant a decision being made as to the comparative virtues of the various schemes. The most promising scheme or schemes might then be investigated further on a reduced vertical exaggeration, which would enable greater detail in side slopes of sandbanks to be reproduced. It would generally be quite practicable to reconstruct a model to a lower vertical exaggeration, the reverse process being difficult, if not impracticable.

Since the writing of the Paper, a model had been constructed in the Manchester laboratory of the estuary of the Parrett, which flowed through Bridgwater into the Bristol Channel. The horizontal scale was 1 : 3,000, and, partly as a matter of general scientific interest, a vertical scale of 1 : 200 was decided upon. Mr. Lacey's formula would require 1 : 208, so that the scale of 1 : 200 was virtually in agreement with his theory. It was found, however, that tidal phenomena were not accurately reproduced with that scale, and, as a result of experiment, the vertical scale was changed to 1 : 260 whilst maintaining the tidal period at that appropriate to the 1 : 200 scale. Mr. Allen did not consider that the behaviour of that model had been in any way superior to that of other models in his experience having much greater exaggerations of scale relative to Mr. Lacey's theory.

* "Construction and Operation of a Tidal Model of the Severn Estuary," pp. 122 and 135. H.M. Stationery Office, 63-78-2, 1933.

Paper No. 5193.

“The Singapore Airport.” †

By REGINALD LEWIS NUNN, D.S.O., M. Inst. C.E.

Correspondence.

Mr. A. G. Gullan referred to the probability of aircraft, in the near future, having a weight of the order of 85,000 lb. and a tricycle undercarriage. He stressed the necessity for allowing sufficient landing space for aircraft of that magnitude and pointed out that at the Heston airport, three runways were to be laid down, each 2,000 yards long with a hard surface. The blind-landing strip was to be 200 feet wide, and the other two 150 feet wide. The ruling factor in the design of such runways was the fact that maximum wheel loads would probably soon reach the order of 17 tons, which was much greater than any wheel loading allowed for in main road design. It appeared that aircraft designers would have to refrain from increasing the size and weight of machines owing to the prohibitive cost of providing aerodromes suitable for the operation of larger craft. Even if alternative airports could be located and constructed, intermediate and emergency fields would be most difficult to find.

Singapore would no doubt serve as a most important calling place in the future development of air transport. It was felt, however, that the aerodrome, as designed, would shortly become too small and the grass surface prove unsuitable for heavy traffic. It was agreed that concrete runways should not be laid until it was absolutely necessary. The size of hangars adopted there would serve for some years to come, but it was considered that hangars would eventually be required to house, say, four machines and would be approximately 300 feet long by 390 feet wide, each end having two openings of 175 feet span. The clear door opening would be of the order of 35 feet.

Could the Author state the type of door adopted at Singapore and the method of their operation? It was considered that electrically-operated doors would become necessary on major airports.

Mr. Gullan noted that the hangars at Singapore were arranged with point lifting facilities. For operational and repair purposes, it would be more satisfactory to provide a travelling crane capable of lifting from 1½ to 2 tons.

Could the Author state the size and thickness of apron slabs and whether they were reinforced or plain? If reinforcement were used the

† Journal Inst. C.E., vol. 12 (1938-39), p. 69 (June 1939).

quantity would be of interest. What was the effectiveness of pre-cast slabs of small dimensions ?

What was the size and thickness of tarmacadam used in the taxi tracks ? It was noted that the drainage of the taxi tracks was provided by "french" drains placed at either edge. The surface of those drains was completed by a final 2-inch bitumen coat to the aggregate. That method of finishing was not considered satisfactory on account of the machines loosening the stones ; in time the whole surface would become loose and the stones would be a serious danger to the aircraft. The surface finish of those drains could be satisfactorily effected by means of 4 inches of good top-soil laid on gravel filling and covered with good turf not less than 4 inches in thickness.

What were the run-off values adopted in the design of the drainage for the paved area and for the grassed areas ?

Was the grass landing ground treated with any fertiliser ? If so, at what rate was the fertiliser spread, and at what season ?

Had any difficulty been experienced in providing a slope of 1 in 15 on the slipway ?

Although the Author stated on p. 76 § that a flat surface provided the best aerodrome, it could be argued that a concave shape was the ideal solution since it gave better facilities for take-off, landing and artificial lighting. It was realized that, with a concave landing ground, the disposal of the water from the centre of the aerodrome would become a rather difficult matter and would require deep excavation at the edges in order to lay the discharge pipes to the correct gradients. The doming of an aerodrome such as that at Singapore provided the best method for draining but had definite drawbacks when landing, taking-off, and lighting were considered.

On p. 77 § the Author stated "The modern flying-boat demands a clear run for take-off of about $1\frac{1}{2}$ mile with free approaches." He added (p. 78 §) : "A minimum depth of 6 feet at extreme low tide was necessary, and to allow a margin the channel was dredged actually to 7 feet 6 inches." It was of interest to note that the French Government at the transatlantic hydroplane base terminus at Biscarrosse-Houtiques had obtained a site giving in all directions take-off runs of 3 kilometres (1·86 mile) with 4 metres (13·12 feet) depth of water.

Mr. J. B. L. Meek noticed that there had been some difficulty with water ponding on the landing surface, and that no details were given of the basis on which the drainage had been designed. Fig. 5, Plate 1 (facing p. 104 §) showed the area divided by two main drains across the diameter of the circle at right angles to each other, and in addition to those there were twelve shorter main drains. Considering the length of one of those

§ Page numbers so marked refer to the Paper (Footnote (†), p. 530).—SEC. INST. C.E.

main drains from the centre of the circle to the circumference, a length of 1,500 feet, it would appear that, deducting the areas which discharged into the shorter drains, it would have to deal with the discharge from an area of approximately 22 acres. The apex level was given as 115.31, and assuming that the invert of the drain was 3 feet below, it would be at 112.31. *Figs. 16* (p. 85 §) showed the invert of a drain where it discharged into the peripheral drain at about 104.90, so that the fall of the drain would be 7.41 feet in 1,500 feet or a little less than 1 in 200. The time of concentration of the rainfall would be only $8\frac{1}{2}$ minutes if the velocity in the sewer were 3 feet per second. Such a short time of concentration indicated a very high intensity of the rainfall in Singapore. Mr. Meek had the record of a storm in Great Britain with an intensity of $5\frac{1}{2}$ inches per hour for a period of $4\frac{1}{2}$ minutes. That was a very exceptional occurrence, and it would appear reasonable to allow for a frequent intensity of 2 inches per hour in Singapore. In view of the fact that it was impossible to drive a peg into the ground, it appeared reasonable to assume 50 per cent. impermeability. Applying those figures to the formula :

$$Q = 60.5 A . r . p,$$

$$Q = 60.5 \times 22 \times 2 \times 0.50 = 1,331 \text{ cubic feet per minute.}$$

That was the discharge which each of the four main radial drains might have to deal with. If it were necessary for the water on the landing ground to disperse quickly after heavy rain, then the sizes would have to be considerably increased. Mr. Meek had had a similar experience of shallow ponding on an aerodrome in Great Britain although the main drains were of ample size, but that trouble was remedied by putting in a few extra cross-drains.

Did the Author think it wise to have an open drain so near to the actual landing-ground if it could have been avoided? 1,000 yards was not a very long runway for the high landing-speeds of modern aircraft, and it would be a source of accident if a 'plane did happen to overrun the ground.

The Author, in reply to Mr. Gullan, stated that he was of opinion that the landing and take-off performance of aircraft engaged on international passenger traffic should be regulated by international agreement. The ever-increasing demands for larger and larger aerodromes were doing a disservice to civil aviation in two directions: firstly, it was very uneconomic, for aerodromes had to be as near as possible to towns, where land was expensive; and secondly, high landing speeds were not conducive to safety. Whilst it was true that enormous advances had been made in safety of flight, he thought still more would be achieved if regulations prescribed that all public transport aircraft had to be capable of controlled flight at a certain minimum speed—say, 60 miles per hour. Such a provision, with its consequent reduction in serious accidents and

aerodrome costs, would greatly add to the popularity of air travel, even though cruising speeds might be somewhat less until aircraft designers developed forms of variable wing surface to a greater extent.

So far as Singapore was concerned, there were no landing grounds within 2,000 miles which offered any better conditions, and it did not seem likely that the Singapore standard would be surpassed in that part of the world for at least a considerable period of time. As Mr. Gullan rightly observed, intermediate and emergency fields suitable for aircraft requiring greater space than that provided at Singapore would be most difficult to find; in Malaya, for example, it would be economically impossible, and the same applied in some of the adjacent countries.

With regard to hangars, those at Singapore were, the Author believed, the only civil aircraft sheds on the England-Australia route capable of housing the "Empire" type flying boat used by Imperial Airways. The hangar doors were in six sections for each 150-foot opening, and were hand-operated. The doors ran on rails laid in the concrete floor. One man could comfortably work the geared drive and could close or open the six sections in less than 2 minutes. A travelling crane would be more useful than point lifting facilities on the roof trusses, but the extra cost did not seem justified, and the additional hangar height of about 10 feet would have been undesirable.

The pre-cast apron slabs were 5 inches in thickness and were not reinforced. Before that pattern was adopted, experiments had been carried out with various types (plain and reinforced) in order to determine the most effective and economical pattern. A few had given way under the very high concentrated load caused by the small high-pressure tires of the "Empire" flying boat beaching-chassis, but no other aircraft had caused any trouble, and the adoption of that type of pavement appeared to have been justified by results.

The tarmacadam used on the taxi strips was of 2½-inch granite to a final thickness of about 3 inches. There had not, as yet, been any trouble with the surface of the "french" drains. Dipping the stones in hot asphalt before placing seemed to hold them together satisfactorily. Some "french" drains which were originally finished with a turf surface had since had the turf removed and replaced by coated stones to improve the "getaway" of surface water. Rainfall intensity at the rate of about 3 inches per hour, with a probable maximum of 8 inches in 24 hours, was anticipated. Apart from dosing a few small areas where the grass did not originally take well, no fertilizer had been used on the landing ground since it was opened for traffic 2 years ago. No trouble had been experienced with the 1-in-15 slipway other than the necessity for periodic cleaning with wire brooms to remove the slime which accumulated between tidal limits.

With regard to the arguments for and against convex and concave surfaces for landing grounds, perfection would no doubt be obtained only

by providing an uphill surface for landing and a downhill run for taking off, but that was not feasible. The convex surface was as good as the concave surface from a flying point of view, and was much better for drainage purposes. No difficulty had been experienced with the lighting; enough light flowed past the dome to illuminate the surface beyond to a sufficient extent, whilst the best illumination occurred where it was most wanted, namely, where an aircraft normally touched down.

With reference to Mr. Meek's observations on the question of drainage, it had been calculated that the nature of the soil when tightly compacted would allow only a very small percolation, and that most of the rainfall would run off the surface. That had proved to be the case in practice. A very fair discharge from the subsoil drainage system had been observed after heavy rains, but there had been no sign so far of overloading. Had the material which was used for reclamation been more pervious, larger sizes of pipe would have been called for. Provided that the grass was kept closely cut, ponding was not serious; the worst case so far observed had been a depth of 2 inches over a small area. Any slight depressions in the surface were levelled up with sand as soon as they were seen; the grass quickly grew up through the sand.

The reason for leaving the perimeter drain open was expense. Any irregularities due to settlement could be corrected more easily in an open than in a covered drain, and it would probably have been necessary to have supported a covered drain on piled foundations.

He wished to make a correction to his Paper: on p. 84 §, lines 30 and 32, the word "Abercrete" should be substituted for "Colorcrete."

§ *Ibid.*

Paper No. 5152.

"The Principles of Drag-Suction Dredging." †

By HERBERT CHATLEY, D.Sc. (Eng.), M. Inst. C.E.

Correspondence.

Mr. N. N. Maas, of Shanghai, who had been immediately in charge, since March 1937, of the dredging operations undertaken by the *Chien She* on the Yangtze bar, referred to an analysis which he had caused to be made of the frequency of density of hopper mixtures obtained by that dredger. The results of that analysis were given in Tables I, II, and III.

TABLE I.—ANALYSIS OF FREQUENCIES OF DENSITIES OF LOADS.

July 8th, 1935–June 30th, 1937.

Density.	Percentage of solids, assuming density of 1·8 in situ material.	No. 1 Cut.		No. 2 Cut.		Total.	
		Number of loads.	Percentage of 3,375.	Number of loads.	Percentage of 1,448.	Number of loads.	Percentage of 4,823.
1·20–1·30	25·00–37·50	977	29·0	174	12·0	1,151	23·9
1·30–1·40	37·50–50·00	1,684	49·9	783	54·1	2,467	51·1
1·40–1·45	50·00–55·13	530	15·7	357	24·6	887	18·4
1·45–1·50	55·13–62·50	156	4·6	108	7·5	264	5·5
1·50–1·55	62·50–68·75	27	0·8	24	1·7	51	1·1
1·55–1·60	68·75–75·00	1	—	2	0·1	3	—
1·60–1·65	75·00–81·25	—	—	—	—	—	—

It should be explained that the channel being dredged was in the form of an obtuse angle, cut No. 2 (the upper leg) being approximately parallel to the general run of the Yangtze South Channel estuary, which ran in a south-easterly direction. Cut No. 1, the more seaward channel, whilst being continuous with cut No. 2, made an angle with it, and ran approximately east-south-east. The dredger, owing to the Sino-Japanese hos-

† Journal Inst. C.E., vol. 12 (1938–39), p. 185 (June 1939).

TABLE II.—ANALYSIS OF FREQUENCIES OF DENSITIES OF LOADS.

July 8th, 1935–August 2nd, 1937.

Density.	Percentage of solids, assuming density of 1.8 in situ material.	No. 1 Cut.		No. 2 Cut.		Totals.	
		Number of loads.	Percentage of 3,501.	Number of loads.	Percentage of 1,672.	Number of loads.	Percentage of 5,173.
1.20–1.30	25.00–37.50	977	27.9	175	10.5	1,152	22.3
1.30–1.40	37.50–50.00	1,697	48.5	793	47.4	2,490	48.1
1.40–1.45	50.00–55.13	581	16.6	382	22.9	963	18.6
1.45–1.50	55.13–62.50	196	5.6	144	8.6	340	6.6
1.50–1.55	62.50–68.75	45	1.3	116	6.9	161	3.1
1.55–1.60	68.75–75.00	5	0.1	61	3.7	66	1.3
1.60–1.65	75.00–81.25	—	—	1	—	1	—

TABLE III.—ANALYSIS OF FREQUENCIES OF DENSITIES OF LOADS.

July 1st, 1937–August 2nd, 1937.

Density.	Percentage of solids, assuming density of 1.8 in situ material.	No. 1 Cut.		No. 2 Cut.		Total.	
		Number of loads.	Percentage of 126.	Number of loads.	Percentage of 224.	Number of loads.	Percentage of 350.
1.20–1.30	25.00–37.50	—	—	1	0.5	1	0.3
1.30–1.40	37.50–50.00	13	10.3	10	4.5	23	6.6
1.40–1.45	50.00–55.13	51	40.5	25	11.2	76	21.7
1.45–1.50	55.13–62.50	40	31.7	36	16.0	76	21.7
1.50–1.55	62.50–68.75	18	14.3	92	41.0	110	31.4
1.55–1.60	68.75–75.00	4	3.2	59	26.3	63	18.0
1.60–1.65	75.00–81.25	—	—	1	0.5	1	0.3

tilities, had unfortunately not been able to work since the beginning of August 1937.

In July 1937, while working in cut No. 2 and using the smallest drag-head with which the dredger was equipped, hopper loads of a uniform brown-coloured silt were recovered continuously for several days with a hopper-mixture density of over 1.50 for 68 per cent. of the total number of hopper loads. (Only once was the figure greater than 1.60.) Those loads were, however, quite exceptional and, as would be seen from the analysis sheets (Tables I, II, and III) from July 1935 until the end of June 1937, only 1.1 per cent. of the total number of 4,823 hopper loads had a hopper-mixture density of over 1.50, whereas 75 per cent. were under 1.40. Not one was over. Even including the high-density loads of July 1937, the number of loads with a hopper-mixture density of over 1.50 for a total number of loads of 5,173 was only 4.4 per cent., the number of loads of density less than 1.40 being 70.4 per cent. Under such conditions, it was difficult to agree with the Author that average hopper-mixture densities of

less than 1.40 were to be considered bad, without condemning that type of dredger altogether for the particular work it had to undertake.

Mr. Maas had carefully studied the records of time taken to fill the hoppers, including those taken on the official trials, and could find no case where loads with density greater than 1.50 had been obtained just filling the hoppers without overflow in 15 minutes. Over the period referred to, when the exceptional silt loads of high density were being recovered, a load of more than 1.55 hopper-mixture density had been obtained, on one occasion in under 19 minutes, inclusive of 1 minute overflow. Assuming the overflow to have made no difference to the density of hopper mixture, and 700 cubic metres of water to have been present in the hoppers before pumping started, that would give 18 minutes' pumping for a pumped mixture load of about 2,500 cubic metres. That would certainly seem to point to a very high density of in-situ material, but no actual measurements of density were available.

In one other case only, a load of just over 1.50 hopper-mixture density had been obtained in 20 minutes, including 5 minutes' overflow. Making the same assumption as before, that would correspond to 15 minutes' pumping time. Again, although the in-situ material density would undoubtedly be high, no records of it were made.

Actually, in both cases, particularly in the latter one, overflow was bound to have had a considerable influence on the density of the hopper mixture.

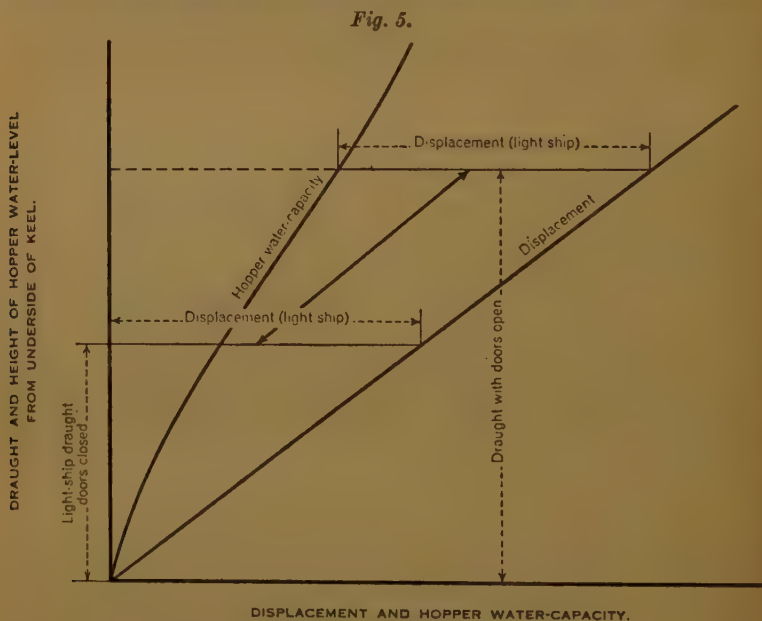
It was easily realized by reference to Tables I, II, and III, as had already been pointed out, that loads of over 1.5 hopper-mixture density were quite rare previous to July 1937, and figures of over 1.55 were practically non-existent. When hopper loads of high density were being obtained in July 1937 no variation in density was noticed with the draught of the vessel. In actual practice, the bed of the river being uneven, certain members of the crew, termed draghead operators, had to be appointed to try and follow the irregularities of the bed by lifting and lowering the draghead accordingly. Those men, in time, had become very expert at following the pump vacuum-gauge indications, and so were able to keep the draghead almost as constantly on the bottom as was possible. The depth, therefore, to which the draghead was buried was continually varying, so that any density-variation effect due to the alteration in draught of the vessel was completely overshadowed by the density variation due to the differences in penetration of the draghead.

It was believed, also, that recent experiments by the University of California * had shown that, in sand-water mixtures, and that presumably held good for mud and/or silt-water mixtures, the head loss above the critical velocity in a pipe-line was no different from that for clear water. The statement, therefore, that the fluid friction for a thick mixture was

* Morrough P. O'Brien and Richard G. Folsom, "The Transportation of Sand in Pipe Lines." University of California Publications in Engineering, vol. 3, No. 7, pp. 343-384.

about 10 times that of clean water did not seem to be substantiated. With the thick mixtures experienced in the *Chien She*, that was to say, with the silt at densities of hopper mixture between 1.55 and 1.60, there was no suggestion of the flow being checked by lack of suction head. Actually the pump ran more smoothly and uniformly without clatter or shuddering. That was due, in Mr. Maas's opinion, to the absence of turbulence. It might be mentioned, in passing, that the high-pressure water feed to the draghead had never been found necessary since the arrangement was tested during the acceptance trials.

The Author had rightly insisted on the design of head and pipe being as streamlined as possible, but Mr. Maas thought that the importance of



that was in the prevention of turbulence rather than in the incidental reduction of fluid friction.

Surely the Author had unnecessarily confused the issue as regards displacement. Mr. Maas thought that the only logical view to take of such a hopper dredger was to look on it as a tight ship with the discharge doors or valves closed, as the case might be, and carrying a cargo of water or mixture. With no water or mixture in the hoppers and doors closed the vessel would float at its light draught, and the displacement would be that of the light ship. When the doors were opened a certain cargo of water would be taken on board and that would increase until equilibrium was reached, or, in other words, until such a draught was attained that the light-ship displacement would lie as a horizontal intercept between the displace-

ment curve of the tight ship and the curve of the hopper water capacity, drawn to the same scale (*Fig. 5*). At the same draught the same ship could not possibly have two different displacements, and the term "rated displacement" was completely unknown to Mr. Maas. If the water and/or mixture were regarded as a load, there could be no possible confusion. Actually, dredger-masters were not satisfied merely to obtain full hoppers, but once that stage was reached they tended to continue as long as possible on overflow, as the Author stated, until a deep draught was recorded. In actual practice, the point along the dredged channel where it was advantageous to turn off to the dumping ground was as much the guide for stopping the pump as anything else, provided, of course, full hoppers had already been secured. That turning point in the channel, however, was seldom reached until some time, ranging from 1 to several minutes, had been spent on overflow. The desire to obtain heavy loads had resulted in excessive longitudinal stresses being developed in the ship, which had shown themselves in cracked plates both in the well walls and outer bottom. The *Fu Shing* had been designed to withstand, without strain, a draught of 21 feet, 3 feet deeper than legend.

Referring to the shape and design of the draghead, Mr. Maas believed that, for the particular service for which *Chien She* and *Fu Shing* had been designed, experience showed that the draghead could be considerably simplified, and the pressure water jets, with their complication of double walls, could be omitted. The suction-release spring-controlled side-valves and extra access doors could probably be omitted also.

Mr. Maas hoped to be able to conduct research by model-experiments to determine the best design of draghead.

The Author, in reply, pointed out that there was no such discrepancy in the density of the hopper loads as Mr. Maas suggested. The Author's figures referred specifically to mud, whereas Mr. Maas's apply to the average conditions on the top of the bar, where there was a certain amount of sand.

The question raised as to the coefficient of rubbing friction was doubtless important in principle. Messrs. O'Brien's and Folsom's Paper had not been seen by the Author at the time that his Paper was written, but in any case it was a matter of interpretation. The energy of sustentation in a rising pipe was as yet by no means clear, and the Author did not think that the allowances made were excessive. Turbulence, friction, and suspension of solids were all interrelated.

With regard to the measurement of hopper loads, the Author deliberately expatiated on the matter of doors open and shut in order to make clear the problems of preliminary pumping out, pumping into a wet hopper, and overrunning the hopper, which vitally affected the density of the load.

In regard to the omission of the pressure water from the drag-head, that was doubtless warrantable in dealing with uniformly smooth mud,

but it had yet to be shown that for heterogeneous materials head-jets were unnecessary.

The Author took the opportunity of noting that the formula in footnote (2) on p. 186 § should be stated as $\frac{s_2 - 1}{s_1 - 1}$, since in practice both s_2 and s_1 was each more than unity.

§ Page numbers so marked refer to the Paper (Journal Inst. C.E., vol. 12 (1938-39), p. 185).—SEC. INST. C.E.

ADDITIONAL ORIGINAL COMMUNICATIONS

RECEIVED BETWEEN THE 1ST SEPTEMBER, 1938, AND THE
31ST AUGUST, 1939.*

TITLES.

- AIRPORTS.—Some Aspects of Aero-Hangar Design. A. M. Hamilton and E. B. Cocks. No. 5,224.
- BRIDGES.—The Kidlington Bridges. I. Kursbatt. No. 5,220.
- DAMS.—Assiut Barrage Remodelling, Egypt. J. E. Bostock. No. 5,222.
- DOCKS AND HARBOURS.—Cochin Harbour Works. A. G. Milne. No. 5,208.
- EXCAVATORS.—The Dragline Excavator. W. Barnes. No. 5,217.
- HYDRAULICS.—On the Hydraulic Problem concerning the Design of Sewage-Storage Tanks and Sea-Outfalls. J. R. Daymond. No. 5,205.
- The Analysis of Flow in Networks of Pipes. R. J. Cornish. No. 5,219.
- HYDROLOGY.—Hydrology of the Yangtze River. Herbert Chatley. No. 5,223.
- RAILWAYS.—Derailments of four-wheeled stock of the Northern Section of the Assam Bengal Railway: Experiments in the Running and Lateral Oscillation of this four-wheeled stock. F. J. Salberg. No. 5,221.
- ROAD-CURVES.—Curve Design for Road Improvements. J. W. L. Barker. No. 5,207.
- SEWERAGE AND SEWAGE-DISPOSAL.—The Main Drainage of Plumstead to Muizenberg. S. S. Morris. No. 5,202.
- The Sewage-Disposal of Delhi. J. A. R. Bromage. No. 5,216.
- See also Hydraulics.*
- STRENGTH OF MATERIALS AND STRUCTURES.—Recent Researches on Combined Stress. J. J. Guest. No. 5,214.
- A Critical Analysis on the Stresses in Helical Springs of Circular Cross Section. J. R. Finnicome. No. 5,225.
- Determination of End-Constraint of Struts by Vibration Methods. H. A. Warren. No. 5,215.
- Graphical Solution of Portal Frames. J. W. H. King. No. 5,226.
- WATER-SUPPLY.—Application of the Experimental Method to the Design of Clarifiers for Waterworks. Robert Walton and T. D. Key. No. 5,213.
- Development of Water-Supply, with reference to Gloucester. G. W. Fuller. No. 5,218.

* Available for reference in the Library; includes Papers awaiting publication.

ENGINEERING RESEARCH.

RESEARCH IN THE ENGINEERING DEPARTMENT AND MINING DEPARTMENT OF EDINBURGH UNIVERSITY, AND IN THE HERIOT-WATT COLLEGE, EDINBURGH; JULY, 1939.

THE Engineering Department of Edinburgh University is equipped for teaching and research in all branches of mechanical and constructional engineering, and the courses in electrical engineering and the work of the Mining Department are arranged in collaboration with the Heriot-Watt College, which is affiliated to the University and is equipped to deal with the specialized work in those subjects. The following notes describe briefly some of the researches that are now in progress.

Researches at the Engineering Laboratories of the University are carried out under the direction of Professor Sir T. Hudson Beare. Extensive tests have been made by Dr. J. B. Todd and Mr. Eric Stevenson on the resistance of spun cast-iron pipes and concrete water-pipes to crushing by external pressure applied across a diameter, and to bursting by internal pressure; this investigation is being carried out on behalf of the Edinburgh Corporation. Much routine testing is carried out on cement and concrete. In connexion with the design of a bridge for Leith docks, comparative tests were made on equivalent welded and riveted sections. Apart from a local failure due to inadequate penetration, the welded specimen proved to be very satisfactory, and the welded construction was successfully adopted for the bridge.

Mr. Leslie Gordon, with the advice of Dr. D. S. Stewart, is carrying out a mathematical study of statically-indeterminate structures. Simplified methods of designing steel and reinforced-concrete framed structures, based on Professor Hardy Cross's method, are being developed. At the same time a convenient practical method of designing reinforced-concrete columns with eccentric loading is being devised. The creep of concrete has been studied by Dr. A. D. Ross*, and Mr. William Dudgeon is now investigating the effects of "dopes" on the tensile and compressive strengths, creep, and other properties of concrete.

In the Heat Engines laboratory, Dr. Maxwell Davidson has adapted a six-cylinder commercial compression-ignition engine for use in an investigation of various problems of ignition, combustion, and heat-flow. Attention is being devoted, firstly, to the effect on indicator diagrams of the length of the connexion between the combustion-chamber and the indicator.

* "The Creep of Portland Blast-Furnace Cement Concrete." *Journal Inst. C.E.*, vol. 8 (1937-38), p. 43 (February 1938).

All six cylinders are provided with normal-pattern connexions for a Farnborough-type indicator, the connecting passage being 3 inches long, and one cylinder is provided, in addition, with a special fitting, enabling the indicator to be about 1 inch from the combustion-space; it is hoped thereby to calibrate the lag on the indicator record introduced by the normal connexions. Thermocouples are being fitted at many points in the engine and exhaust-system, including one in the exhaust-port close to the exhaust-valve, so as to allow a thorough investigation to be made of temperatures and heat-flow during operation. Particular attention will be given to the distribution of work in the engine and to the effect of the water-jacket temperature on the performance.

Mr. Karl Herbstritt is studying the effect of the air-inlet temperature and also of the oil temperature on the process of combustion in an oil engine made by the National Gas Engine Company, of 8-inch bore and 16-inch stroke. The purpose of this research is to study, chiefly: (1) the combustion process, (2) the heat losses to jacket and to exhaust, and (3) the mechanical efficiency, and to compare the results obtained by preheating the air and by preheating the fuel.

Dr. J. B. Todd and Mr. Charles Patterson are developing a gravity-fed oil-fuel burner, atomization being effected by an annular air-blast. Successful operation with a low consumption of air has been achieved with the first experimental type, and a smaller burner is now being constructed to enable calorimetric tests to be undertaken. The effectiveness of the design depends largely on the reduction of hydraulic losses; various grades of oil have been successfully burnt, preheating being used with the more viscous oils.

Several researches are being carried out in the Electrical Engineering laboratories of the Heriot-Watt College, under the direction of Professor M. G. Say. The operation of the "metadyne" converter is being studied by means of an experimental machine built up from standard dynamo parts, and so arranged as to enable the effects of changes in construction to be examined. For the analysis of short-circuit effects in alternating-current supply networks, a calculator has been constructed in which a resistance-net supplied with direct current is used to stimulate the actual impedance-network under operating conditions.

A study is being made of the phenomenon of negative phase sequence voltages, which appear when a balanced three-phase system becomes unbalanced under fault-conditions. The possibility of employing the effect in the design of protective systems is being examined.

The important physical problem of oscillations in circuits with non-linear parameters is being investigated mathematically in collaboration with the Department of Mathematics.

In an electro-encephalograph constructed for use in the Clinical Department of the Royal Infirmary, Edinburgh, the minute electrical potentials developed by the brain are magnified by a thermionic amplifier

with an overall voltage amplification of approximately 10^6 , resistance-coupled pentodes being employed with a paraphase output circuit operating a cathode-ray recorder. The principal difficulty in the design of the amplifier has been to obtain stability whilst maintaining the response down to extremely low frequencies. The staff of the Electrical Engineering Department has co-operated with the manufacturers and the Infirmary personnel in investigating causes of instability.

An *ad hoc* investigation of unusual interest is at present being carried on to develop a continuous method of testing brass wire for the presence of iron in a proportion as small as 1/20 of 1 per cent., which renders the wire brittle and therefore unsatisfactory for use in manufacturing wire-mesh for papermaking machinery. One method that is being tried consists in passing the wire continuously through a coil constituting one arm of a balanced alternating-current bridge circuit; it is hoped that the change in inductance produced by a minute percentage of iron in the wire may enable its presence to be detected. Preliminary tests with pure and impure wire suggest that the method may be practicable, but it has not yet been possible to obtain a sufficient sensitivity.

The Department of Mechanical Engineering of the Heriot-Watt College, under Professor A. R. Horne, is not at present carrying out researches other than testing and investigations in connexion with the strength of materials.

Several researches and investigations are at present in progress in the Mining Department of the University of Edinburgh and Heriot-Watt College, Edinburgh, under the direction of Professor W. H. McMillan. The effects and avoidance of glare in underground lighting, and improvements in the light distribution from miners' electric cap lamps have been studied, and the results so far obtained have recently been published *; work on these problems is being continued. The application of photographic photometry to mine lighting problems is also being examined, and much experimental and testing work has also been carried out on the detection of firedamp by electrical methods; these investigations are also being extended.

* Prof. W. H. McMillan and S. Holmes, "Improvements in Light Distribution from Miners' Electric Cap Lamps." Trans. Inst. Min. E., vol. xevi (Part 1), p. 28.

Prof. W. H. McMillan and S. Holmes, "Glare in Underground Lighting; A Preliminary Study." *Ibid.*, vol. xevii (Part 1), p. 196.

OBITUARY.

SIR HENRY JAPP, K.B.E., was born at Montrose on the 6th June, 1869, and died on the 11th April, 1939, at The Orchard, Bedford Park. He received his early education at Montrose Academy. This was followed by a technical education at University College and later at the Finsbury Technical Institute, London. Although best known as a civil engineer, he served his 5 years' apprenticeship with Messrs. Caledon Engine Works, Dundee, builders of land and marine engines, and mill machinery. The next 3 years were spent in the drawing offices of Messrs. The Thames Iron Works, Millwall, and Messrs. Humphreys and Tennant, Deptford. In 1895, 2 years after the completion of his studies, he was appointed engineer-in-charge of constructional work on the Surrey Commercial Docks, and later on the Great Northern and City Railway, London, the contractors being Messrs. S. Pearson and Son, Ltd. Sir Henry remained with this firm for 20 years. In 1904 he became director and managing engineer of the Pennsylvania East River Tunnels, New York. In 1915 he was appointed Director of Production for the British War Mission in the United States. For his work in his department he was created C.B.E. in 1917, and advanced to K.B.E. in 1918.

After the Great War, Sir Henry was associated with many great constructional enterprises, including the completion of the Prince of Wales Dock, Workington; the King George V Graving Dock, Southampton; the Dover Train Ferry Dock; and the jetty and power-house foundations for the Ford Motor Works, Dagenham. His most recent outstanding work was the preparation of a report, in conjunction with Dr. David Anderson and Mr. B. L. Hurst and under the auspices of the Lord Privy Seal, on the problem of air-raid shelters. This report was published in the form of a White Paper, in February, 1939.

Sir Henry was elected an Associate Member of The Institution in 1895 and was transferred to the Class of Member in 1899. He delivered the Institution Lecture to Students in the Session 1934-35*. He was also a Member of the Engineering Institute of Canada and of the American Society of Civil Engineers, before which he read several Papers. The Whitworth Society elected him President for the year 1936-37.

Sir Henry married twice. By his first wife, Elizabeth, daughter of David Hodge, of Dundee, he had two sons and two daughters. In 1915 he married Kathie, daughter of R. W. Sutherland, of Montreal.

*"Modern Methods and Plant for Excavations." The Inst. C.E., 1935.

EDWARD JAMES WAY was born on the 28th July, 1866, and died in London on the 6th February, 1939. After 2 years' training at King's College, he completed a 4 years' pupilage with Messrs. Wignes and Harland in 1886. He was a Transvaal pioneer as resident engineer to the Transvaal Exploration and Land Company, and consulting engineer of the Morgenson Gold Mining Company.

In 1892 he became general manager of the Eastleigh Syndicate at Klerksdorp. Shortly afterwards he became chief assistant to Mr. J. H. Hammond, the well-known American mining expert and general manager of the George Goch Gold Mining Company. After being appointed general manager of the Kleinfontein group of mines in 1898, he was appointed consulting engineer in 1906 and was responsible, during the 27 years he worked with the group, for a large amount of reconstruction work after destruction during the South African War and for new work later. In 1924 Mr. Way left the Union to take up practice in London as a consulting engineer.

He was elected an Associate Member of The Institution in 1901, and was transferred to the class of Member in 1908. He served as Member of Council resident in South Africa from 1917 to 1919. He was President of the South African Institution of Engineers in 1914-15, and was at one time Chairman of the Technical Committee and Mines Trial Committee of the Transvaal Chamber of Mines. He presented to The Institution in 1906 a Paper on Stamp-Mill Reduction-Plant¹, for which he was awarded a Telford Gold Medal.

He married Kathleen Caroline Berry, by whom he had one son.

¹ "Stamp-Mill Reduction-Plant of the New Kleinfontein Company, Limited, Witwatersrand, Transvaal." Minutes of Proceedings Inst. C.E., vol. clxviii (1906-07, Part II), p. 252.

NOTE.—The Institution as a body is not responsible either for the statements made, or for the opinions expressed, in the Communications published.

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